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A NOTE ON GUTTMAN'S LOWER BOUND FOR THE NUMBER OF COMMON FACTORS[1]

By Henry F. Kaiser
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Guttman's classic lower bound for the number of common factors is extended to the completely general case where communalities may lie in the closed interval from zero to one.

Guttman [2] has established a lower bound for the number of common factors. His result is as follows. Given R, the correlation matrix of order n, let $R_0 = R - I$, and G^2 = the diagonal matrix of squared multiple correlations of each variable with the remaining $n-1$: then the number of non-negative latent roots of $R_0 + G^2$ is a lower bound to r, the rank of the matrix $R_0 + H^2$, where H^2 is a diagonal matrix of communalities. Guttman's treatment is for the usual case : $0 < h^2_j < 1$, $j = 1, 2, \ldots, n$. It is the purpose of this note to extend his result to the general case, viz. $0 \leqq h^2_j \leqq 1$, $j = 1, 2, \ldots, n$.

Let the *positive index* of a real symmetric matrix be the number of positive latent roots of the matrix, and similarly, the *non-negative index* the number of non-negative latent roots. The crux of Guttman's finding lies in the following lemma.

Lemma. Let B be any real symmetric matrix, C a non-singular real Gramian matrix, and A their sum, so that

$$A = B + C \; ; \tag{1}$$

and let a be the positive index of A, and b the non-negative index of B : then

$$a \geqslant b. \tag{2}$$

We write the identity

$$(R_0 + H^2) = (R_0 + G^2) + (H^2 - G^2). \tag{3}$$

Since $g^2_j < h^2_j$ for $0 < h^2_j < 1$, $j = 1, 2, \ldots, n$ ([1], pp. 92–93), then, letting k^* be the non-negative index of $(R_0 + G^2)$, we may associate $(R_0 + H^2)$ with A, $(R_0 + G^2)$ with B, and $(H^2 - G^2)$ with C in Guttman's Lemma, thus

$$r \geqslant k^*, \tag{4}$$

Guttman's lower bound for r.

In general, however, we may only say $g^2_j \leqslant h^2_j$ [3], and the Lemma is then inapplicable since $(H^2 - G^2)$ is no longer necessarily non-singular. We shall now show that Guttman's conclusion holds for the general case, except when $R_0 + G^2$ is singular.

[1] This research was supported by the United States Air Force under Contract No. AF41 (657)–76, Professor Charles Wrigley, principal investigator.

Let $D = dI$ be an arbitrary positive scalar matrix of order n. We rewrite (3) as

$$(R_0 + H^2) = (R_0 + G^2 - D) + (H^2 - G^2 + D), \tag{5}$$

and since $(H^2 - G^2 + D)$ is necessarily positive definite, we may apply the above Lemma, and say

$$r \geqslant k_d^* \tag{6}$$

where k_d^* is the non-negative index of $(R_0 + G^2 - D)$. If m_s is any latent root of $(R_0 + G^2)$, then $m_s - d$ is the corresponding latent root of $(R_0 + G^2 - D)$. By choosing d less than the smallest positive latent root of $(R_0 + G^2)$, then

$$k = k_d \tag{7}$$

where k and k_d are the positive indices of $(R_0 + G^2)$ and $(R_0 + G^2 - D)$ respectively. (Unfortunately, we cannot in general choose d such that $k^* = k_d^*$ since $(R_0 + G^2)$, if singular, would have a latent root or roots equal to zero.) Obviously $k_d^* \geqslant k_d$, and using (6) and (7),

$$r \geqslant k.$$

Thus, for all appropriate H^2, the number of positive latent roots of $(R_0 + G^2)$ is a lower bound for the number of common factors.

Guttman [2], in his original paper, has applied his Lemma to show that if we have $\hat{g}^2_j < g^2_j$, $j = 1, 2, \ldots, n$, weaker lower bounds to the h^2_j, then $\hat{k}^* \leqslant k^*$, a weaker lower bound to r for $0 < h^2_j < 1$. Through a development similar to that of the preceding paragraph, we may generalize his result so that if $\hat{g}^2_j \leqslant g^2_j$, $j = 1, 2, \ldots, n$, then $\hat{k} \leqslant k$, a weaker lower bound to r for all H^2.

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[3] Guttman, L. (1956). 'Best possible' systematic estimates of communalities. *Psychometrika*, XXI, 273–285.

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INTELLIGENCE AND SOCIAL MOBILITY

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The main thesis of the following paper is that, in a highly organized society, the discrepancies between the general intelligence of the children and the occupational class into which they are born is bound to produce a large and fairly constant amount of ' basic mobility ', quite apart from any deliberate changes in the political or educational structure of the society.

Since the correlation between the intelligence of fathers and sons is only about 0·50, it is evident that, when classified according to their occupational status, (i) the mean intelligence of the children belonging to each class will exhibit a marked regression towards the general mean, and (ii) the intelligence of the individual children within each class will vary over a far wider range than that of their fathers. These deductions are fully confirmed by tables compiled to show the actual distribution of intelligence among adults and children belonging to the various occupational categories. It follows that, if the frequency distribution within the several classes is to remain constant (and still more if there is to be an increasing degree of vocational adjustment among later generations), a considerable amount of social mobility must inevitably take place, involving between 20 and 30 per cent of the population. Approximate estimates are attempted of both the actual and the ideal amounts. Data obtained from the after-histories of schoolchildren, followed up in later life, are analysed to ascertain the main psychological causes tending to produce a rise or drop in occupational status.

I. The Class Distribution of Intelligence

Aim. In the course of a recent discussion on the mental differences between social classes [19, 20, 21] I argued that the apparent differences between the class-means for general intelligence were to be explained partly by the effects of social mobility in transferring abler individuals from lower classes to higher and duller individuals from higher classes to lower, and partly by the manner in which inherited or innate differences are transmitted from one generation to another. Several sociological writers, however, have questioned both these suggestions, or at any rate the way in which I assumed the two processes had actually operated. In this paper, therefore, I propose to offer more detailed evidence to support the interpretations I put forward, and at the same time to answer, so far as I can, the various objections raised against the arguments which I advanced on these various points ([19], pp. 22f, section on ' Social Mobility ').

The data which I shall analyse are drawn from two overlapping inquiries, or rather two series of inquiries: (i) cross-sectional surveys of pupils in London schools, initiated primarily for the purposes of educational or vocational guidance

and selection; (ii) longitudinal studies of backward, gifted, and normal pupils, followed up into adult life chiefly to check the accuracy of the assessments and recommendations made while the children were still at school [4, 26]. The surveys and the subsequent inquiries were carried out at intervals over a period of nearly fifty years, namely, from 1913 onwards; and much of the data is due to the willing cooperation of numerous collaborators, particularly teachers and social workers in the service of the London County Council and colleagues or senior research-students working for the National Institute of Industrial Psychology, to all of whom I am deeply indebted.

Points of Agreement. It may help to clarify the issues involved if I begin by summarizing the major points on which both my critics and myself would, I fancy, be in general agreement.

1. During the period covered by our inquiries the population, from which our samples are drawn, and to which we intend our conclusions to apply, greatly increased in numbers, though at a diminishing rate. Both the increase itself and the diminution in the rate of increase were a continuation of processes that had been going on during the preceding half-century. Thus in 1860 the total population of England and Wales was nearly 20 million; in 1910 it was almost twice as large—36 million; and in 1960 it was 45 million [10, 11, 24].

2. During the last half-century the proportional number of children in the population steadily declined and that of the elderly steadily increased. In 1910 31 per cent of the population were boys and girls of school age (i.e. under 15) and 7 per cent men or women over 60; in 1960 only 22 per cent were of school age and 14 per cent over 60.

3. Among the lower working classes (unskilled manual labourers) both the birthrate and the deathrate were appreciably higher than among the semi-skilled, skilled, or professional classes. The number of live births per married woman averaged about 3·8 among unskilled labourers and only 1·8 in the professional classes. The differences in the mortality rates were much smaller, averaging 13·1 per 1000 among the unskilled labourers and only 10·8 per 1000 in the professional classes. For birth and death alike the absolute rates and the class differences have both appreciably diminished during the period in question. The excess of birthrate over the deathrate has been by far the most important cause of the increase in the population [11, 24].

4. During the period for which information is available there has been no great change in the average level of general intelligence. The results of the second Scottish survey indicated an actual improvement in the average score with the tests employed [7]; but I myself believe, as Sir Godfrey Thomson suggested in his preface to the report, that this was an artificial and somewhat misleading result, due partly to increased familiarity with the tests and methods of testing [8]. On the whole, a survey of the relevant evidence would appear to suggest an actual but comparatively slight decline during the period in question, approximating to a drop of 1 or 2 I.Q. points per generation [5].

5. The amount of individual variation about the average level of intelligence has apparently remained fairly constant; certainly it has not declined [6, 16]. When we compare the printed tables giving the standard deviations for complete age-groups, we usually find that in the earlier surveys it is about 12 or 13 I.Q. points and in the later as much as 16 or even more. But the apparent increase is in all probability to be explained by the fact that the later test-scales are greatly improved, and as a result decidedly more discriminative. Where the same tests have been used the figures show no appreciable change.

6. There are appreciable differences in the average level of intelligence in the different socio-economic classes, and in spite of the remarkable improvements in material and cultural conditions, the differences have altered hardly at all during the period in question [1, 3, 16].

On all these points further research and more exact information is undoubtedly required. But I believe that, as a rough provisional statement, what I have said would be accepted by most social psychologists.

Points of Disagreement. The issues on which disagreement has been most strongly expressed are those relating to the genetic hypothesis. Dr. Floud and Dr. Halsey, for example, deny that the apparent differences between the class-means for general intelligence are in any degree due to innate differences; and both contend instead for " a hypothesis of near-randomness in the social distribution of innate intelligence ". This implies that the means for all the classes would be approximately the same. Many of their colleagues have also argued that even " the apparent differences in intelligence between individuals, whether adults or children, result not from genetic causes but solely or mainly from environmental conditions ". Dr. Halsey, however, is prepared to admit that individuals may vary in innate ability; but the model he has put forward to explain how such differences are in his view transmitted and redistributed diverges widely from mine [21]. In particular he criticizes both the amount of social mobility which I had assumed and the length of time over which I assumed it had operated; his view, like that of many other social writers[1], apparently supposes that social mobility is a comparatively late phenomenon, the result more especially of recent social and educational reforms.

II. Alternative Methods of Analysis

Correlational Analysis and Variance Analysis. Many of the foregoing criticisms arise, I fancy, very largely from the fact that the method which I adopted in the investigations cited differed considerably from those adopted for psychological researches on heredity in the past. Most psychologists have discussed the problems of genetics in terms of the correlational procedures popularized by Karl Pearson; the investigations of my coworkers and myself were based mainly on an analysis of variance using the techniques applied by Ronald

[1] Most of them ignore genetic influences altogether: see the interesting papers in *Population Studies*, IX, pp. 72–81, 82–95, XI, pp. 123–136, 262–8.

Fisher. Unfortunately such methods still seem unfamiliar to the majority of psychologists and sociologists working in this country.

The analysis of variance has numerous merits with which statistical investigators are already well acquainted; but in the field of genetics it has one special advantage over the older correlational techniques. In nearly all the earlier statistical studies heredity itself was commonly conceived as 'the tendency of like to beget like'; hence the correlation coefficient, as a measure of likeness, seemed the obvious tool. On the Mendelian theory, however, genetic influences are responsible, not only for resemblances between members of the same family, but also for differences, i.e. for individual variations. Genetic variability within families receives little or no attention from those psychological critics who still accept the Pearsonian view, and think mainly in terms of correlations; yet, as we shall see in a moment, variability within families forms one of the chief causes of mobility. Moreover, it was to a large extent the exclusive reliance on correlational analysis which was responsible for the abnormally low estimates which Pearson and his followers reached for the influence of environment. Variance techniques make it far easier to give due weight to environmental influences, and to the further complications which result from the fact that environment and heredity so often work in the same direction.

In the present paper I shall, so far as possible, avoid unfamiliar methods and formulae. Nevertheless, because I believe that researches undertaken in the near future should be deliberately planned to permit the application of these newer and more efficient techniques, I will first attempt a brief explanation of the type of procedure that would seem most appropriate, and at the same time indicate how it is related to the more familiar correlational procedures. This may to some extent help to elucidate several of the points in my recent paper which the critics have either questioned or misunderstood.

The Factorial Analysis of Variance. Let us start, rather on the lines of Fisher (*op. cit. inf.*, pp. 210f.), with the simplest type of situation—that in which only two independent components of variance are involved. To make the problem concrete let us consider the case of n identical twins, reared in different environments, and tested or assessed for some form of educational or occupational efficiency, which we may plausibly suppose to be the result of both genetic (g) and environmental factors (e). If x_i denotes the assessment for the ith individual, r a correlation coefficient, and s^2 an estimated variance, we may write

$$x_i = g_i + e_i \qquad (i = 1, 2 \ldots n) \qquad \text{(i)}$$

Squaring and summing we obtain

$$\Sigma x^2_i = \Sigma g^2_i + 2\Sigma g_i e_i + \Sigma e_1{}^2,$$

$$\text{and therefore } s^2_x = s^2_g + 2r_{ge} s_g s_e + s^2_e,$$

$$\text{or } s^2_x = s^2_g + s^2_e, \qquad \text{(ii)}$$

if it can be assumed that the foster-homes have been chosen in a way quite unrelated to the intellectual level of each child's own family.

If we adopt a correlational procedure, the appropriate coefficient will be the *intra-class* correlation. Accordingly, following Fisher (with a slight change of notation) let us put

$$r_{ii} = \frac{G}{G+E}, \qquad \text{(iii)}$$

where G and E denote the genetic and environmental variances respectively. Now, as Fisher shows, we can obtain an unbiased estimate of r_{ii} from the two equations

$$n(1-r)s^2_x = \Sigma(x_i - \bar{x}_j)^2 = ns^2_w, \qquad (i = 1, 2 \ldots 2n) \qquad \text{(iv)}$$

$$(n-1)\,(1+r)s^2_x = 2\Sigma(\bar{x}_j - \bar{x})^2 = (n-1)s^2_b, \qquad (j = 1, 2 \ldots n) \qquad \text{(v)}$$

where x_i (as before) denotes the assessment of the ith individual, $\bar{x}_j$ the mean of the family to which that individual belongs, s^2_x the estimated total variance, s^2_w the variance ' within families ', and s^2_b the variance ' between families ', n the number of families, and $2n$ therefore the number of twins, and r the correlation between the twins' intelligence[1].

To estimate the relative size of the contributions of G and E, however, we need two further equations. Fisher shows in the course of his discussion that

$$\sum^{mn}(x - \bar{x}_j)^2 = n(m-1)E, \qquad \text{(vi)}$$

$$\text{and } \sum^{n}(\bar{x}_j - \bar{x})^2/(n-1) = G + E/m, \qquad \text{(vii)}$$

where m is the number in each family (with twins $m = 2$). Substituting from (iv) and (v), and then solving for $G = s^2_g$ and $E = s^2_e$, we obtain

$$G = \tfrac{1}{2}(s^2_b - s^2_w), \qquad \text{(viii)}$$

$$E = s^2_b. \qquad \text{(ix)}$$

These then are the equations we require; and I have ventured to call the whole procedure the ' factorial analysis of variance '.[2] On substituting in equation (iii) from equations (viii) and (ix) we have for the coefficient of correlation

$$r_{ii} = \frac{s^2_b - s^2_w}{s^2_b + s^2_w}, \qquad \text{(x)}$$

which sums up the relation between the results of the two alternative procedures —correlation and analysis of variance—in the simplest conceivable case.

In the foregoing problem—that of identical twins brought up in separate homes—both the variance within the family due to genetic influences and the effects of environment so far as it operates in the same direction as the genetic influences could be safely ignored. If we wish to estimate the former, we can

[1] Fisher, *Statistical Methods for Research Workers* (5th ed., 1934, chapter VII, ' Intra-Class Correlation and Analysis of Variance '. The formula for r is given on p. 212, and the equations for the sum of squares ' within families ' and ' between families ' will be those given in Table 39 not Table 38: cf. also [4], pp. 675f. and [14], pp. 106f.

[2] This type of analysis has wide applications in psychometrics, and may also be extended to the study of interactions: see, for example, this *Journal*, VIII, p. 116.

take ordinary siblings brought up from birth in the *same* environment, e.g. orphanages and other residential institutions (cf. [3], pp. 90–91); and if we want to determine the effects of any correlation between environment and heredity, we can either reintroduce the correlational term r_{ge} or calculate the additional variance directly by the method described in the earlier paper [14].

Multiple Cross-Classifications. To deal with more complex situations the foregoing techniques can readily be extended to problems involving a multi-dimensional classification. In such cases, it may be noted, both the algebraic solutions and the arithmetical calculations become much simpler if the successive classifications are dichotomous. Thus, in dealing with genetical problems, it would certainly be desirable to take into account temperamental and motivational tendencies (m) as well as cognitive abilities (a). Since a general factor underlies each, we may for most purposes, treat each as supplying the basis for a further two-fold classification. In studying the influence of social class we must cross-classify both the genetic factors and the environmental according to the variations in family (f) and in social class (c): this would mean that our simple dichotomous equation (equation (i) above) must now be rewritten

$$x = g_c + g_f + e_c + e_f, \qquad \text{(xi)}$$

and, if we wish to include motivational factors as well as cognitive, we must be prepared to work with eight variables, g_{ac}, g_{mc}, g_{af}, etc. In either case the derivation of the formulae will proceed much as before. But, by deciding in advance which variables we will include and which we will exclude (e.g. by arranging to keep certain conditions constant) and by avoiding so far as possible interactions or intercorrelations between the variables retained, many of the complications may be eliminated or at least reduced to insignificance.

Incidentally, we may note that with an analysis of variance it is not necessary (as it would be with a correlational procedure) to assume that the social classes themselves must be expressed by measurable quantities (e.g. by income) or ranked in linear order (e.g. in terms of prestige), as in fact has been the custom in many sociological inquiries. The method thus avoids the difficulty experienced by investigators of mobility who have endeavoured to assess or rank rural occupations on the same scale as urban and industrial[1]. The calculations would become simpler still if we were content, with several recent investigators (Lipset and Bendix [23], for example), to reduce the occupational classification in either case to a twofold division, namely, manual and non-manual.

[1] Glass [13], for example, in his study of social mobility in Britain, attempts to classify urban and rural populations together, and in this he is followed by L. Livi and K. Svalastoga in Italy and Denmark respectively—agricultural countries where a unidimensional classification of this kind ceases to be plausible [9]. Most other investigators classify the urban and the rural populations separately [15, 17, 18]. Even in the case of urban occupations much of the data available is expressed in terms of occupational categories which it would be very difficult to rank; in such cases, therefore, the analysis of variance, or (if Pearsonian techniques are preferred) the calculation of contingency coefficients, is far more appropriate than the calculation of correlations.

In principle, therefore, the questions with which we are concerned are essentially problems in multivariate analysis. Multivariate analysis can take several forms; and, according to the specific nature of the question we wish to answer, we may use either factor analysis, regression analysis, or discriminant analysis. But, once again, in any future research it is desirable that the investigator should keep explicitly in mind from the outset the kind of statistical techniques that are suitable for his problem and his data, and having made his choice, plan his inquiry accordingly.

In the following discussion, which is intended merely as a pilot inquiry, I shall, to begin with, confine myself primarily to assessments for general intelligence and leave motivational factors to a later section. I shall compare assessments for adults and children drawn always from the same families; but I shall adopt a moderately elaborate occupational classification. The data are too crude and limited for a detailed examination by a full analysis of variance. Moreover, in this paper it is my purpose to keep, so far as possible, to the simplest and most intelligible methods of comparison, relying largely on the percentage methods favoured by sociologists themselves. But the differences revealed, I fancy, will be sufficiently striking to lend strong support to the conclusions drawn.

III. Frequency Distributions for Adults and Children

Sources of Data. In studying the distribution of intelligence among the different occupational classes it is in my view desirable to examine, not only (as is usually done) the class-means, but the entire frequency distributions. Accordingly in Tables I and II I give frequencies both for adults and for children. For the children the bulk of the data was obtained from the surveys carried out from time to time in a London borough selected as typical of the whole county. The methods by which the assessments for intelligence were made have been described in earlier papers and in L.C.C. Reports [3, 5, 16]. For the boys who belong to the highest occupational classes, drawn for example from families who would not ordinarily send their children to Council schools, much of the data was collected in the course of work on vocational guidance at the National Institute of Industrial Psychology. The data for the adults was obtained from the parents of the children themselves. Usually our more immediate purpose was to secure practical estimates of both the average level and the range of intelligence required in the commoner types of occupation. In addition, however, when working with backward children we often wanted to see how far the backwardness was a family characteristic. And at all levels an incidental aim was to secure material for studying the problem of mental inheritance. For obvious reasons the assessments of adult intelligence were less thorough and less reliable.

The Occupational Classification. The occupational classification is much the same as that used in previous reports. It has been described by Carr-Saunders and Caradog Jones in their book on *Social Structure in England and*

Wales ([2], Table XXXI, p. 56). Unlike the classification used in the more recent studies of social mobility it is based, not on prestige or income, but rather on the degree of ability required for the work. Class I includes those engaged in the highest type of professional and administrative work (university teachers, those of similar standing in law, medicine, education, or the church, and the top people in commerce, industry, or the civil service); class II consists of those engaged in lower professional or technical work (including most teachers, men of business, and executive clerks in the higher grades); class III of those working in intermediate types of clerical, commercial, or technical work; class IV includes those ordinarily classified as skilled workers, but it also contains an appreciable number who are engaged in commercial or industrial work of an equivalent level; class V consists of semi-skilled workers and those holding the poorest type of commercial position; class VI of unskilled labourers, casual labourers, and those employed on coarse manual work. It will be noted that the numbers in the higher groups or classes are far smaller than those in the lower. These subdivisions were in fact chosen because at the outset of our work we had in mind the proportionate numbers of children (*a*) who were transferred to Central Schools (about 12 per cent), (*b*) who were awarded junior county scholarships and transferred to what were then called secondary (i.e. grammar) schools (about 3 per cent), and (*c*) who were of exceptionally high intelligence and for the most part in attendance, not at a council school, but at one of the older public schools or at a preparatory school of similar type (about 0·3 per cent); and we wanted the occupational classification to tally so far as possible with the educational classification.

In constructing the tables the frequencies inserted in the various rows and columns were proportional frequencies and in no way represent the number actually examined: from class I the number actually examined was nearer a hundred and twenty than three. To obtain the figures to be inserted (numbers per mille) we weighted the actual numbers so that the proportions in each class should be equal to the estimated proportions for the total population. Finally, for purposes of the present analysis we have rescaled our assessments of intelligence so that the mean of the whole group is 100 and the standard deviation 15. This is done because the results of so many intelligence tests nowadays are expressed in terms of conventional I.Q.'s conforming to these requirements.

IV. Amount of Mobility

The Distribution of Adults. From the figures set out in the last column of Table I it will be seen that there are appreciable differences between the average levels of intelligence in the various classes. The average for the highest class of all—those holding the highest professional or administrative appointments—is practically 40 I.Q. points above the general level. The differences between the means for the last three classes are much smaller, largely because the numbers are far greater.

TABLE I. DISTRIBUTION OF INTELLIGENCE ACCORDING TO OCCUPATIONAL CLASS: ADULTS

		50–60	60–70	70–80	80–90	90–100	100–110	110–120	120–130	130–140	140+	Total	Mean I.Q.
I.	Higher Professional									2	1	3	139·7
II.	Lower Professional							2	13	15	1	31	130·6
III.	Clerical				1	8	16	56	38	3		122	115·9
IV.	Skilled			2	11	51	101	78	14	1		258	108·2
V.	Semiskilled		5	15	31	135	120	17	2			325	97·8
VI.	Unskilled	1	18	52	117	53	11	9				261	84·9
	Total	1	23	69	160	247	248	162	67	21	2	1000	100·0

TABLE II. DISTRIBUTION OF INTELLIGENCE ACCORDING TO OCCUPATIONAL CLASS: CHILDREN

		50–60	60–70	70–80	80–90	90–100	100–110	110–120	120–130	130–140	140+	Total	Mean I.Q.
I.	Higher Professional						1		1	1		3	120·8
II.	Lower Professional				1	2	6	12	8	2		31	114·7
III.	Clerical			3	8	21	31	35	18	6		122	107·8
IV.	Skilled		1	12	33	53	70	59	22	7	1	258	104·6
V.	Semiskilled	1	6	23	55	99	85	38	13	5		325	98·9
VI.	Unskilled	1	15	32	62	75	54	16	6			261	92·6
	Total	2	22	70	159	250	247	160	68	21	1	1000	100·0

But still more striking is the wide range of individual differences within each class. With a normal distribution the range for an unselected group of 1000 cases would be (as in fact appears from the table) nearly 100 I.Q. points—i.e., from about 50 to 150; for an unselected group of 100 cases it would be about 75 to 80 I.Q. points. We should naturally expect, however, that the members of the occupational classes will form selected groups, and that their range and standard deviation will therefore be appreciably diminished. In point of fact the standard deviation within the various classes averages 9·6 (i.e. rather less than two-thirds the standard deviation of the entire group, 15). Hence the range for 100 cases would still be nearly 50 I.Q. points (as a glance at classes III to VI will confirm). Indeed, in the lowest class of all—that of unskilled workers—some of the brightest members actually display greater intelligence than the dullest members in class II, the ' lower professional '. The correlation between intelligence and occupational class therefore is by no means perfect. If we attempt to estimate it on the assumption that both distributions are in fact normal, it works out at just over 0·74. However, since the correlation must be far from linear, its precise numerical value as thus calculated can have little meaning.

The fact that the correlation is far from perfect must not be taken to imply that the duller members of the higher classes and the brighter members of the lower classes are all of necessity instances of vocational misfit. No doubt, they sometimes are. But frequently specific abilities or disabilities, and still more often qualities or infirmities of character and temperament, will fully account for the apparent discrepancies.

The Ideal Redistribution. In order to determine what is the maximum amount of interchange that ideal conditions could possibly permit, let us suppose that vocational adaptation depends solely on intelligence. Then in terms of the I.Q. scale the borderlines between the several occupational classes would be 141, 127, 115, 103, and 90 respectively, and there should be no overlapping between the successive categories. If we now reclassify the actual data for adults according to these new borderlines, we obtain the distribution set out in Table III. The number who are placed in occupations corresponding with their intelligence is shown in semi-bold. Before calculating the percentages let us pool the first three classes together to form a single group which is mainly non-manual; and let us combine the next two to form a group of skilled workers (predominantly but not entirely manual), and leave the lowest class as it is. With this threefold rearrangement (Table V) we find that only 55 per cent of the population could be regarded as correctly placed if intelligence were the sole criterion: nearly 23 per cent are in a class too high, and, with a perfect scheme of vocational selection, ought to be moved down: 22 per cent are in a class too low, and would have to be moved up.

The Distribution for Children. When we turn to the data for children (Table II), we observe that the differences between the class-means are much smaller. The average intelligence of the children in the higher groups has

TABLE III. DISTRIBUTION OF INTELLIGENCE ACCORDING TO OCCUPATIONAL CLASS: ADULTS

Rescaled

	VI 50– 91	V 91– 103	IV 103– 115	III 115– 127	II 127– 141	I 141+	Total
I					2	**1**	3
II			1	15	**14**	1	31
III	1	15	38	**56**	12		122
IV	16	86	**114**	38	4		258
V	53	**178**	84	10			325
VI	**191**	46	21	3			261
Total	261	325	258	122	32	2	1000

TABLE IV. DISTRIBUTION OF INTELLIGENCE ACCORDING TO OCCUPATIONAL CLASS: CHILDREN

Rescaled

	VI 50– 91	V 91– 103	IV 103– 115	III 115– 127	II 127– 141	I 141+	Total
I			1	1	1		3
II	1	4	11	9	**6**		31
III	11	28	51	**20**	12		122
IV	46	66	**75**	62	8	1	258
V	91	**122**	84	23	5		325
VI	**112**	105	36	7	1		261
Total	261	325	258	122	33	1	1000

TABLE V. ADULTS: PERCENTAGE IN EACH GROUP WHOSE INTELLIGENCE IS BELOW, ABOVE, OR EQUIVALENT TO THAT OF THEIR OCCUPATIONAL CLASS

	Below	Equivalent	Above	Number
Class I–III	46·2	45·5	8·3	156
Class IV–V	26·6	50·1	23·3	583
Class VI	—	73·2	26·8	261
Total population	22·7	55·4	21·9	1000

TABLE VI. CHILDREN: PERCENTAGE IN EACH GROUP WHOSE INTELLIGENCE IS BELOW, ABOVE, OR EQUIVALENT TO THAT OF THEIR OCCUPATIONAL CLASS

	Below	Equivalent	Above	Number
Class I–III	75·5	16·8	7·7	156
Class IV–V	34·8	34·3	30·9	583
Class VI	—	42·9	57·1	261
Total population	32·1	33·5	34·4	1000

fallen almost half-way towards the mean of the whole population; similarly that of the children in the lower groups has risen by a similar proportion. There is, in short, an overall regression averaging 0·52 (cf. [1], [3]). The figure is very close to the value we should expect on the assumption that the correlation between fathers and sons was due chiefly to multifactorial inheritance with assortative mating and incomplete dominance. If anything, the coefficient is slightly higher than we might expect on these grounds alone [cf. 14]. Hence environmental influences may perhaps have contributed to increase it; but, if so, the contribution must be extremely small.

The phenomenon just noted has sometimes been termed ' biological regression '; and several sociological and psychological writers have claimed that this tendency is responsible for ' the steady progress which ' (so they hold) ' most populations are continually undergoing from a state of individual diversity to one of increasing individual equality ', so that in the more highly civilized communities the distribution of intelligence is approaching ' near-randomness ' as regards both classes and individuals. This interpretation appears to have been adopted as a corollary to the theory of ' blended inheritance ' to which the majority of psychologists and sociologists still adhere[1]. A few writers, however, who recognize that the data obtained from successive generations reveal no evidence whatever for this alleged tendency towards equality, have also postulated a biological ' egression *from* the mean ', which, they argue, " balances regression *towards* the mean " [22]. For this there is no need. With multifactorial inheritance this ' conservation of variance ' is what we should expect.

When we look at the distribution of children's intelligence *within* the several occupational classes and compare it with that of their fathers' (Table I), we see at once that, so far from progressing towards equality, the amount of individual difference has actually increased. The standard deviation has gone up from 9·6 to 14·0, not far short of the standard deviation for the whole population (15·0). Or, to put it in another way, the range for 100 individuals selected according to occupational class has increased from 50 I.Q. for adults to nearly 75 I.Q. for their children.

One incidental consequence of this increase in variability is the appearance of bright children among the offspring of dull parents in the lower occupational classes and of dull children among the offspring of highly intelligent parents in the upper occupational classes. Consider, for example, the lowest occupational class of all. Among the adults only 20 persons out of 261 have an intelligence above the general average; among the children as many as 76, nearly four times as many—a discrepancy of 56. Dr. Floud, and others who hold as she does that differences in intelligence are due wholly to environmental advantages or disadvantages, can hardly maintain that the high level reached by these 76 boys

[1] This corollary from the theory of blended inheritance is mathematically deduced, and empirically disproved, in an earlier issue of this *Journal* (X, p. 56). The empirical disproof of the corollary is perhaps one of the simplest and most convincing arguments against the doctrine of blending. Clarke's ' egression ' is apparently a substitute for Darwin's ' spontaneous variation '.

—all children of unskilled workers—results from the superior advantages which their home environments confer. But equally, those who adopt the traditional theory of blended inheritance, would find it quite impossible to explain the higher intelligence of these children in terms of their heredity. On the Mendelian hypothesis, however, such apparent anomalies are exactly what we should anticipate if the amount of a child's intelligence is determined mainly, or at any rate largely, by his genetic constitution, and if that in turn is the result of a chance recombination of parental genes ([14], p. 97).

Similar arguments hold good for the marked discrepancies discernible in the upper part of the distribution. In the first three occupational classes, for example, we see that among the adults only 9 out of 156 had an intelligence below the general mean, among the children as many as 39. Here again the increased numbers would be almost inexplicable on the environmental theory, but a natural consequence of the Mendelian theory of polygenic inheritance.

Consequences of the Intergenerational Changes. We have seen that two changes result from the comparatively moderate correlation that obtains between the intelligence of parents and their children: (i) the mean intelligence of the children belonging to each occupational class deviates far less than the mean of the parents from the average for the population as a whole, and (ii) the intelligence of the individual children within any one class varies over a far wider range than that of their parents. Moreover, unless their effects are in some way counteracted, both these changes will be cumulative. After about five generations the differences between the class-means would virtually vanish, and the proportional range within each class would spread out almost as widely as the proportional range of the population as a whole[1].

Now all the evidence shows (p. 5 above) that in point of fact, during the period with which we have been concerned, the occupational distribution of intelligence has remained fairly constant from one generation to the next, and it appears likely to do so in the immediate future. If therefore, when they are grown up, the children of Table IV are to exhibit the same distribution as the adults of Table I, it follows that a considerable number will have to move into a fresh occupational class. Some will go up the social ladder by one or more rungs; others will go down. One of our chief problems therefore is to assess the extent of this migration. For this purpose it will be helpful to begin by rearranging the figures for the children according to the method we have already adopted for the adults (Table III above).

Maximum Mobility. Table IV shows the distribution of the children with the scale for intelligence subdivided afresh so that the lines of division shall correspond with those we should expect between the different occupational classes if occupational efficiency depended solely upon intelligence. As before, let us group together classes I, II and III to form a non-manual group, and

[1] This is a simple mathematical corollary. Allowing for assortative mating and partial dominance, the correlation between occupation and intelligence after n generations would sink to approximately $0{\cdot}74 \times \frac{1}{2} \times (2/3)^{n-1}$: (See [14], p. 116, eq. 23).

classes IV and V together to form a group of skilled workers, leaving class VI as it stands to form a group of unskilled workers. Then, assuming intelligence to be the sole criterion, it appears (as a little mental calculation will quickly show) that in the highest group 75 per cent of the children have an intelligence below the minimum that would be needed if they were to become efficient members of the occupational group into which they were born; on the other hand, in the lowest group 57 per cent have an intelligence well above the meagre amount required for an unskilled worker (see Table VI). In the entire sample over a third of the children have an intelligence which would apparently fit them for a higher occupational class than that of their fathers, and rather less than a third have an intelligence which would be more appropriate for a lower class.

These figures give a rough indication of the amount of movement upward or downward from one class to another which the children would have to undergo when grown up in order that the type of work they secured corresponded with their degree of intelligence—always assuming that intelligence was the sole criterion. In point of fact we know that nothing like this amount of movement actually takes place; and the figures, of course, merely indicate the *maximum* degree of mobility that is theoretically conceivable.

Actual Mobility. Let us now return to the question of fact. The ideal and most direct procedure would be to plan a longitudinal study of a large and representative sample, following up the children from school to middle life. A complete inquiry of this kind has so far never been attempted. Both in America and in this country follow-up studies have been undertaken for certain selected groups—the gifted or the backward; but for our present problem these provide at most only supplementary or confirmatory evidence. We are obliged therefore to fall back on the alternative procedure commonly adopted in similar situations; and, instead of comparing the same group at two widely diverging intervals of time, we shall compare two different groups of widely divergent ages.

Our present data supply us with two such samples. These are comparable, since the adults are the parents of the children. However, there is a difference of 28·4 years between the average age of the children and the average age of the adults; and, as we have seen, during that amount of time there would have been a variety of changes in the population. Our method of reducing the figures observed to numbers per 1000 should sufficiently allow for the change in the absolute size of the population. The differential birthrate may have entailed some slight modification in the mental quality of the population; but, in the space of three decades only, the extent of the change would, as we have seen, be almost negligible. The effects of the deathrate are largely ruled out by the fact that we have taken children who have survived to school age. Although between 1911 and 1951 the proportion of men and women over 65 very nearly doubled, this was offset by a decline in the number of boys and girls under 15: and the proportional number of males of employable age has remained much the same. The type of work available has changed appreciably: the number of those engaged in manufacturing and in professional and administrative work of various kinds

has increased; the number engaged in agriculture, in the extractive industries (mining, quarry, etc.), in domestic work, and in the distributive trades has diminished; moreover, the amount of prestige attaching to different types of occupation has altered. Nevertheless, these further changes are hardly relevant to our present problem, as we have formulated it, although in a more intensive study the bearing of all the varying conditions I have mentioned should undoubtedly be systematically examined.

Assuming then that the data in our two samples are reasonably comparable, our primary task is to determine what kind of compensating change would be necessary to bring the frequency distributions for the children (Table II) into conformity with the frequency distribution for the adults (Table I). Let us look first at the lowest occupational class of all—the unskilled workers (class VI). Among the children, it will be remembered, as many as 57 per cent have an intelligence above what is required for work of this type as against 27 per cent of the adults (Tables V and VI). Hence $(57-27)=30$ per cent of the children will presumably move up to a higher occupational class as they grow up. Similarly $(75-46)=29$ per cent of the upper group—that comprising classes I, II and III—will move down. In the intermediate group—classes IV and V—the changes both upward and downward will be smaller. Thus, as a comparison of the last lines of the two tables suggests, the over-all mobility will be at least $(55-33)=22$ per cent. This figure I regard as indicating the minimum amount of mobility—the amount that is required to maintain what (if I may borrow a phrase from the astronomers) might be called a ' steady state '[1]. It constitutes what may be termed ' basic mobility'.

[1] The foregoing data and the analysis I have here attempted will, I hope, dispose of one of the strongest objections urged by Dr. Halsey [21] against the arguments brought forward by Miss Conway and myself in our endeavour to account for the wide differences in average intelligence shown by the different socio-economic classes. Dr. Halsey's criticism was that the round figures assumed for social mobility in setting up our genetic model were far too high. But our object then was of course very different from our present purpose. We merely wanted to show that, with a comparatively small amount of interchange between the several classes, a society which started from primitive conditions in which the average intelligence in the different classes was practically the same would, in the course of subsequent generations, be differentiated in such a way that the differences between the mean levels of intelligence corresponded pretty closely with those at the present day. For this purpose we deliberately assumed in our hypothetical model an amount of mobility well below that which we believed had actually occurred in order to forestall incidental criticisms on this point. In view of the figures given by sociologists themselves Dr. Halsey's criticism seemed rather surprising. But we now hope that the foregoing analysis will show that our postulated figure was well below the most probable minimum.

It has been objected that any figure for social mobility, like that given above, is bound to vary with the lines of division adopted in classifying occupations. However, as long as the basis of the classification remains unaltered, changes in the lines of division will not seriously affect the estimated figure unless the lines of division become so few and the resulting classes so large that the amount of movement is obscured. Indeed, if we imagine the various occupations to be graded according to difficulty in such a way that the distribution of the employees is approximately normal, then in theory, provided we know (i) the correlation between the intelligence of the employees and grade of the occupation, and (ii) the correlation between the intelligence of the employees' children and that of the employees themselves, the amount of mobility required to keep the population constant could be determined from the properties of the bivariate frequency distribution.

However, as we have already seen, there was, at the time when the occupations of the fathers were recorded, considerable room for improvement in the degree of adjustment between the capabilities of the individuals and the type of employment they followed. Moreover, during the last forty years or so, as several researches in the field of vocational guidance have shown, the degree of adjustment has appreciably increased, and apparently is still increasing. Hence the actual amount of mobility is probably much greater than that which just suffices to maintain the *status quo ante*. If we may trust the most thorough of the recent inquiries [13], the overall amount of mobility would appear to be in the neighbourhood of 29 per cent—well above our minimum for a steady state, but still far below what the ideals of vocational suitability would require. Much the same figure was obtained from our analysis of after-histories (p. 19)—viz. 31 per cent.

As the reader will realize, the foregoing deductions deal only with a very limited aspect in a very limited interpretation of the rather ambiguous phrase ' social mobility '. Ordinarily the discussion of mobility treats a rise in social status as implying something more than a mere rise to a type of occupation which requires a higher I.Q. than the occupation followed by one's father[1]; and the value attached to different types of occupation as a goal for the ambitious youngster varies widely from group to group, from individual to individual, and from one period to another. Nor is intelligence the only factor which determines whether or not the ambitious youngster will succeed in achieving the vocational career at which he aims.

I propose therefore in conclusion to glance at two or three other factors which might be expected to influence the kind of occupational status which persons of varying intelligence are likely to attain, and consider what is their relative importance and how far they could affect the inferences already drawn.

V. Causal Factors

Data. Although opinions have been freely expressed about the conditions which facilitate social advancement and still more often about those which are thought to obstruct it, surprisingly little factual evidence has been obtained. The teaching of Samuel Smiles and his Victorian doctrine of ' Self-Help ' has long since faded from memory, though his biographical illustrations are by no means valueless. However, during the past fifty years the popular tendency has been to place an increasing emphasis on the external or social factors and less on the personal or psychological. But here it is principally the latter which I should like quite briefly to examine.

[1] As I have pointed out elsewhere, in their definitions of social class and social status different writers have relied on a wide variety of criteria ([12], pp. 37f. and refs.). The problems to which discussions of this type give rise are most readily handled by means of factor analysis. It turns out that all the criteria are positively, and indeed closely, correlated: so that a general factor must underlie them all. Hence the ideal way of allocating persons to an appropriate social class would be to use a system of weights based on a multiple regression equation. The use of such a technique of course implies that the necessary data have been collected for all the individuals concerned.

As part of the longitudinal studies of gifted and of backward children attending London County Council schools my colleagues and I have followed up into later life a large number of cases, not only of these somewhat exceptional types, but also of normal or average children who were treated as control groups. We now possess fairly detailed data for just over 200 ordinary children who have already reached an age when it is possible to say either that they have already moved out of their original class, in one direction or the other, or else that it is now practically certain that they will never do so. We have similar numbers for pupils who formerly attended central schools or won junior county scholarships as well as for pupils who were educationally subnormal. By using fractional weights for the figures obtained from these various subgroups we can compile a composite group of males which shall be reasonably representative of the total population. It includes many of the older children in the group discussed in section II—those whom we have been able to trace and follow up in their after-school life; but it includes others omitted from that group owing to lack of adequate data about their parents' abilities. For each of the sub-groups we have the following relevant information, obtained (except for vi) mainly when the children were at school: (i) the occupational class of the fathers at the time the children were born; (ii) assessments and descriptions of the home background, and particularly of the attitude of the family towards the child's social advancement; (iii) the child's own attitude, and particularly his industry, ambition, and educational and vocational aims; (iv) his level of intelligence, based on tests duly checked with the teachers and corrected where necessary; (v) his educational record (more especially his admission to a grammar school or its equivalent); (vi) his occupation when last visited.

In view of the complexity of the problem and the limitations of the data let us begin by an analysis in terms of crude percentages. For this purpose we may divide the whole composite group into three portions—(*a*) those who have remained in their original occupational class, (*b*) those who have moved up, and (*c*) those who have moved down. Similarly, it will simplify matters to reduce the assessments for intelligence to a threefold classification—(*a*) an intelligence equal to that required in the individual's original occupational class (the class of his father), (*b*) an intelligence above it, and (*c*) an intelligence below it. The other assessments can be reduced to a twofold classification, viz. (*a*) above the median and (*b*) below the median[1].

Results. The main results are shown in Table VII. The following conclusions may be drawn.

1. Of the children with an intelligence *below* the minimum required for the occupational class into which they were born none rose above it, and about a third (or rather more) dropped to a lower class. On the other hand, of those

[1] For much of the data in this section, and most of the calculations, I am deeply indebted to my former colleagues, Miss J. L. Hastings, Miss E. Davenport, and Mr. R. M. Weldon.

who had an intelligence *above* the maximum required for their original occupational class nearly 60 per cent failed to rise.

2. Very few of those who were assessed as lacking in adequate motivation rose above their original class. Indeed, poor motivation was more likely than poor intelligence to contribute to a fall. On the other hand, good motivation was less certain to secure a rise.

3. A good home background, though helpful particularly during the earlier educational stages, was less effective in securing a rise than either high intelligence or strong motivation. Nor was a bad home background so fatal as seems to be commonly assumed. Nearly a quarter (24 per cent) of those who suffered from unfavourable home circumstances in childhood nevertheless succeeded in rising out of their original class.

TABLE VII. PSYCHOLOGICAL FACTORS INFLUENCING OCCUPATIONAL MOBILITY

	Intelligence			Motivation		Home Background		Educational Achievement	
Mobility	P	A	G	P	G	P	G	P	G
Up	0	12	41	2	36	24	29	18	34
Stationary	64	67	49	47	51	40	44	34	52
Down	36	21	10	51	13	36	27	48	14
	100	100	100	100	100	100	100	100	100

	Intelligence and Motivation Combined		Intelligence, Motivation, and Home Background Combined		Intelligence, Motivation, Home Background, and Educational Achievement Combined	
Mobility	P	G	P	G	P	G
Up	0	66	0	70	0	72
Stationary	32	29	23	23	18	21
Down	68	5	77	7	82	7
	100	100	100	100	100	100

Note.—P=Poor, A=Average, G=Good. The tables for 'combined' qualities include only those cases in which *all* the qualities specified were 'Poor' or 'Good'.

4. The achievement of grammar school status or (during the pre-war period) the award of a junior county scholarship, by no means sufficed to guarantee a rise in occupational class, though it often proved an important step in the child's gradual ascent. However, an appreciable number succeeded in working their way up to a higher occupational level in spite of a total lack of any formal education beyond what the elementary school could provide.

5. Two-thirds of those who have both high intelligence and strong motivation are likely to achieve an occupational rise. In fact nearly all who achieve a rise have this double characteristic. The addition of a good home background

is a further advantage; in the case of the child of the lower classes what chiefly count are the social aspirations, the ambitious aims, and the constant urging that often characterize the more earnest working-class parents; with children from higher levels it is rather the intellectual and cultural character of the home that helps. The addition of a grammar school education does not greatly increase predictability[1]: this is because most children who have good intelligence, good motivation, and good home backgrounds are pretty sure to win their way to a grammar school. Those who, despite high intelligence and good motivation, dropped to a lower occupational class were for the most part victims of ill health, either mental or physical. As the tables indicate, there was actually an increase in this type of failure among those who in addition enjoyed a good home and a good education; the increase, as the case-histories would show, is accounted for by the larger number who break down from nervous ill health.

There are many other possible factors of a somewhat miscellaneous kind which have not been included in the foregoing summary—e.g. the variations in the openings available at different times or in different regions, the effects of a wife and family, or of friends, acquaintances, or patrons able to help and wielding personal influence. From time to time we encountered evidence of such factors; but as determinants of individual mobility they seemed to be of much less importance than those we have discussed.

An Analysis by Factors and Variances. The relations between the five main variables can be roughly expressed by means of correlation coefficients. For this purpose we have assessed mobility in terms of the degree of movement as well as the direction, allotting ± 1 point for a movement to the class above or below, ± 2 points for a movement over two classes, and so on. The correlations obtained are shown in Table VIII. The correlations between social mobility and the various causal conditions are product-moment coefficients; the correlations of the causal conditions with each other are averaged tetrachorics.

The raw correlations between social mobility and the four main causal conditions differ but little in magnitude; but the partial correlations (last row of Table VIII) differ appreciably. The correlation with intelligence is by far the highest of the three (0·38); the correlation with motivation is somewhat smaller (0·29); and the correlations with home background (0·17) and educational achievement (0·05) almost negligible. These figures thus fully confirm the conclusions already reached. The partial regression equation for predicting mobility is

$$S = 0{\cdot}346I + 0{\cdot}272M + 0{\cdot}158H + 0{\cdot}149E,$$

where S denotes social mobility, I intelligence, M motivation, H home background, and E educational achievement. The multiple correlation is 0·628.

[1] Education, and particularly the type of education (e.g. entrance at a public school followed by Oxford or Cambridge), are unquestionably influential in determining a rise at the highest levels of all; but in a survey of the total population such cases appear by comparison few in number and exceptional in type.

This rather modest value is no doubt due to the fact that, as we have seen, various minor factors may be operative in individual cases[1].

Table IX gives the results of the factor analysis. The matrix of correlations between the four causal conditions was first subjected to a group factor analysis, the lines of division between the groups being determined by a preliminary bipolar analysis. The correlations between social mobility, etc., and the resulting factors were then computed by the usual formula. In determining the meaning of the factors we have relied partly on the case-histories of a few

Table VIII. Correlations between Social Mobility and Related Conditions

	1	2	3	4	5
1. Social Mobility	—	0·481	0·402	0·396	0·378
2. Intelligence	0·481	—	0·133	0·236	0·413
3. Motivation	0·402	0·133	—	0·376	0·162
4. Home Conditions	0·396	0·236	0·376	—	0·363
5. Educational Record	0·378	0·413	0·162	0·363	—
Social Mobility (partial correlation)	—	0·379	0·286	0·174	0·047

Note.—The partial correlations give the correlation between social mobility and the condition specified when the effects of the other three conditions are held constant.

Table IX. Correlations of Social Mobility and Related Conditions with Group Factors

Factor	I	II	III	IV
1. Social Mobility	0·443	0·417	0·294	0·166
2. Intelligence	0·782	0·000	0·405	0·000
3. Motivation	0·000	0·731	0·329	0·000
4. Home Conditions	0·302	0·514	0·000	0·287
5. Educational Record	0·528	0·216	0·000	0·323

typical individuals who have obtained exceptionally high or exceptionally low factor measurements for each of the factors. The first factor appears to be essentially an intellectual factor; and the second a factor of incentive. The last two are based on a bipolar factor which apparently distinguishes variations that are mainly genetic from variations that are mainly environmental But the precise interpretation of factor III remains somewhat obscure[2].

[1] The method is similar to that adopted in an earlier memorandum and subjected to considerable criticism: for a reply see [6], pp. 278f.

[2] Factors I and II must each of them be partly determined by genetic characteristics. Factor III seems to imply some overlap or linkage between the genetic characteristics that make for intelligence and stability of character respectively—perhaps due merely to the fact that intelligence is an ingredient of stability, or perhaps due to some selective conditions influencing the genetic basis of both.

Between them the four factors contribute about 48 per cent of the total variance for mobility: of this, factor I contributes nearly 20 per cent, factor II 17 per cent, factor III 9 per cent, and factor IV barely 3 per cent.

Owing to the imperfect nature of the data and the methods of calculating the correlations it seems very doubtful whether much value can be attached to the figures thus obtained. We give them chiefly in the hope that their shortcomings may encourage fresh investigators to plan a more systematic set of longitudinal studies, with the method of analysis kept carefully in view from the very outset, and so in the end reach a more reliable basis of comparison[1].

VI. Summary and Conclusions

1. As a convenient criterion for vocational adjustment it is assumed that, if the available occupations are grouped in order of the difficulty of the work they entail, and if the men engaged on them are grouped in order of intelligence, then there should be a perfect correspondence between the two series. Judged by this criterion it appears that well over 20 per cent of the male adults in this country have a higher intelligence than is requisite for the work they are doing and that about the same number have an intelligence which is inadequate. Many of the discrepancies, however, are accounted for by individual differences in qualities of personality or character or in special abilities or aptitudes relevant to the work concerned.

2. Owing to the imperfect correlation between the intelligence of parents and the intelligence of their children the discrepancies between the children's intelligence and the occupational category of the parents are still greater. This follows from the multifactorial theory of inheritance, and is amply confirmed by the data here examined. The figures indicate that an overall mobility of about 22 per cent is needed to keep the distribution of intelligence approximately constant from one generation to another within each occupational category. If the distribution of character-qualities could also be taken into account, a still higher figure would no doubt be obtained. There is, moreover, considerable evidence to suggest that the degree of general vocational adaptation is improving; and, partly for this reason, it is estimated that the total amount of intergenerational mobility must be nearer 30 per cent.

3. Of the various causal factors affecting the individual's rise or fall in occupational status differences in intelligence and motivation appear to be the most influential. Differences in home background and in education seem to exercise a secondary or supplementary influence, but without the basis of the first two they are of little effect.

[1] I am much indebted to Miss Howard for assistance with the calculations involved in this section.

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SUBJECTIVE DISCRIMINATION AS A STATISTICAL METHOD

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The following paper describes the use of human discriminators to test the hypothesis that there is no difference between two groups of items. Four methods of making a test on the basis of such discriminations are discussed; and the procedure is applied to experimental data.

I. Introduction

The use of statistical tests in psychology has the disadvantage that, for their significance levels to be meaningful, the procedures must be defined before the data are examined. This implies that the hypotheses to be tested must be formulated in advance; accidental discoveries are suspect. The difficulty is overcome by a two-stage procedure. Data from a pilot experiment (or a randomly chosen part of the whole data) are examined. An explanatory hypothesis is then exactly specified, and an appropriate test of it is made in the main experiment (or in the previously unexamined part of the data).

A method which integrates these two stages is that of ' subjective discrimination '. This method will be discussed, and applied to the data of Table I. The table gives five successive decrements in performance (expressed in terms of the percentage of missed signals) at a vigilance task carried out by two groups of subjects, namely, (i) $S_1, \ldots, S_6$, who started the task at 10 a.m. and (ii) $S_7, \ldots, S_{12}$, who started it at 2 p.m. It is of psychological interest to know whether there is any significant difference in the pattern of decrements between the two groups.

Table I. Performance Decrements (D) at a Vigilance Task : G_1 *at* 10 *a.m.*, G_2 *at* 2 *p.m.*

		D_1	D_2	D_3	D_4	D_5
G_1	S_1	3	2	−6	6	−4
	S_2	12	10	−2	13	−4
	S_3	8	−7	−4	4	−3
	S_4	−5	−1	6	15	−3
	S_5	3	3	2	−19	−7
	S_6	8	7	−5	0	−16
G_2	S_7	9	−5	1	0	6
	S_8	15	2	0	−4	17
	S_9	38	−7	−1	−6	2
	S_{10}	26	−6	11	−5	6
	S_{11}	15	2	3	−6	−4
	S_{12}	12	9	−1	0	4

II. The Method

To maintain some generality let us suppose we have two groups of items, G_1 and G_2, m and n in number respectively. (For the data of Table I the items will be graphs of the decrement scores, one for each subject.) The $m+n$ items are presented in a random array to each of J judges, who are the 'subjective discriminators'. They are asked to sort the items into two groups, m and n in number, so that members of the same group shall be more similar than members of different groups. The groupings are recorded. The choice of a similarity criterion is left to the judges, but in most cases it would clearly be useful to ask them to describe their criteria.

III. General Analysis

Denote the items in G_1 by $I_1, \ldots, I_m$ and those in G_2 by $I_{m+1}, \ldots, I_{m+n}$. Denote the jth judge's m-group and n-group by J_{m^j} and J_{n^j} respectively.

(*a*) *Single judge.* A test of the null hypothesis (that the judge is unable to discriminate between G_1 and G_2) is provided by testing association between them and the judge's groups.

(*b*) *More than one judge. Method I* (applicable only when $m \neq n$). Let a_i be the number of judges who place I_i in their n-groups. The null hypothesis that G_1 and G_2 are indistinguishable means that, in independent repetitions of the experiment (or observation) which gave rise to G_1 and G_2, the numbers $a_1, a_2, \ldots, a_{m+n}$ would have a symmetrical distribution with respect to the suffices $1, 2, \ldots, m+n$, that is, the numbers would be distributed in a manner unrelated to the division into G_1 and G_2. The alternative hypothesis of discriminability of G_1 and G_2 would result in the numbers $a_1, \ldots, a_m$ being smaller than $a_{m+1}, \ldots, a_{m+n}$. Therefore suitable tests of the null hypothesis are provided by (i) a randomization test of the difference between the sets $\{a_1, \ldots, a_m\}$ and $\{a_{m+1}, \ldots, a_{m+n}\}$, using $a_1 + \ldots + a_m$ as the test criterion, or (ii) a randomization test with the a_i replaced by their ranks, which is equivalent to the Mann–Whitney rank test.

Method II (a modification of Method I for the case $m = n$). When $m = n$, the decision as to which group shall be denoted by J_{m^j} and which by J_{n^j} is arbitrary. One of the items, say I_s, is selected by the experimenter as a standard; and, for each j, J_{m^j} is defined to include I_s. With the arbitrariness thus removed the numbers a_i are calculated as for Method I (now $a_s = 0$). Provided the selection of I_s is made on a criterion independent of the division into G_1 and G_2, the numbers $\{a_i / i \neq s\}$ will, on the null hypothesis, have a symmetrical distribution: so an appropriate test of the null hypothesis is the same as for Method I, except that the two sets of numbers are now $\{a_1, \ldots, a_m\} - \{a_s\}$ and $\{a_{m+1}, \ldots, a_{m+n}\} - \{a_s\}$. It is clear that, if a standard were chosen about which judgements are erratic, the test would be weakened. To avoid this, s may be chosen so as to maximize

$$\max_{i \neq s} a_i - \min_{i \neq s} a_i.$$

If more than one value of s maximizes this expression, the one used may be chosen from these at random. This criterion for the choice of s satisfies the condition of being independent of the division into G_1 and G_2, and maximizes the agreement between judges in a particular sense. No doubt other reasonable criteria also exist.

Method III (m and n unrestricted). Let a_{ij} be the number of judges putting I_i and I_j in the same group. The null hypothesis that G_1 and G_2 are indistinguishable means that, in independent repetitions of the experiment (or observation) which gave rise to G_1 and G_2, the numbers $\{a_{ij}/i=1, \ldots, m+n; j=1, \ldots, m+n;\ i \neq j\}$ should have a symmetrical distribution with respect to the suffices, that is, they should be distributed in a manner unrelated to the division into G_1 and G_2. There are $\frac{1}{2}(m+n)\ (m+n-l)$ different pairs of curves of which $\frac{1}{2}m(m-1)+\frac{1}{2}n(n-1)$ are from curves in the same group, G_1 or G_2. Under the alternative hypothesis of discriminability, we would expect the a_{ij}'s from the latter pairs to be larger than the a_{ij}'s from the remaining pairs. The following randomization test is therefore appropriate for testing the null hypothesis.

Let $G_1^* - G_2^*$ denote one of the $(m+n)!/m!n!$ ways of conceptually dividing the $m+n$ items into two groups of sizes m and n. Let S^* be defined by

$$S^* = \sum_{\substack{I_i \in G_1^* \\ I_j \in G_2^*}} a_{ij}.$$

Then the significance probability is the proportion of the S^*'s which equal or are less than the S^* for the division into G_1 and G_2. To assist the calculation of S^*, the following relation may be used:

$$S^* = \sum_{j=1}^{J} \{b_j^*(m-b_j^*) + b_j^*(n-b_j^*)\}$$

where b_j^* is the number of items in G_1^* and J_n^j simultaneously.

Method IV (m and n unrestricted). Proceed as for Method III but use

$$S^* = -\sum_{j=1}^{J} \{b_j^{*2} - (m-b_j^*)(n-b_j^*)\}^2$$

$$= -\sum_{j=1}^{J} \{mn - (m+n)b_j^*\}^2.$$

Here S^* is proportional to the sum of J chi-squared statistics for testing the association between $G_1^* - G_2^*$ and $J_m^j - J_n^j$ $(j=1, \ldots, J)$.

Comparison of the Methods. Method I is computationally the simplest, and is easy to apply even when m and n are large. However, the work required to select the standard item in Method II is not very great. Methods III and IV would usually require the assistance of an electronic computer, the exception being when all the judges discriminate nearly perfectly. For all the methods there is a limit to the significance level as the number of judges is increased. The limits are $m!n!/(m+n)!$ for Method I, $m!(m-1)!/(2m-1)!$ for Method II,

$m!n!/(m+n)!$ for Methods III and IV when $m \neq n$, $2.m!m!/(2m)!$ for Methods III and IV when $m=n$. In practice the significance level may not increase as J increases; one good judge may do better than a group of judges.

No power comparisons will be attempted here. There is a twofold difficulty in such comparisons. First, it is not easy to specify a sensible alternative hypothesis; secondly, the calculations would be complex. However, it is clear that Method I will not be powerful when $(m-n)/m$ is small and positive, and the judges have probability near $\frac{1}{2}$ of putting G_2 in their m-group.

IV. Application to Experimental Data

Twelve graphs of the data (one for each subject) were presented in random array to each of nine judges. The groupings made are recorded in Table II, where they are distinguished by 0's and 1's. On applying Method II for simplicity, it can be seen that the graph of S_1 is a suitable standard. The Mann–Whitney test yields a significance probability of less than 0·01.

Table II. Results for the Nine Judges

		S.H.	J.L.	H.D.	A.B.	R.W.	E.P.	M.S.	I.H.	I.B.
G_1	S_1	0	0	0	0	0	0	0	0	0
	S_2	0	0	0	0	1	0	0	0	0
	S_3	0	0	0	0	0	0	0	0	0
	S_4	1	0	0	0	1	0	0	0	1
	S_5	1	1	0	1	1	1	0	0	1
	S_6	0	0	0	0	0	0	0	0	0
G_2	S_7	1	1	1	1	0	1	1	1	1
	S_8	0	1	1	1	1	1	1	1	0
	S_9	1	1	1	1	1	1	1	1	1
	S_{10}	1	1	1	1	1	1	1	1	1
	S_{11}	1	1	1	1	0	1	1	1	1
	S_{12}	0	0	1	0	0	0	1	1	0

Alternative methods of analysis do not achieve the same level of significance. With the assumption of multivariate-normality and equality of covariance matrices over the population of experimental subjects, Hotelling's T^2 can be used to test the null hypothesis that G_1 and G_2 have the same means. Using the method of Sect. 5.3.3 of Anderson's *Introduction to Multivariate Analysis*, the differences prove non-significant even at the 5 per cent level. The most outstanding *single* feature of the data is the difference between the two groups under D_1. With a two-tailed Mann–Whitney test this is significant at $P=0{\cdot}008$. However, allowance must be made for the way the feature to be tested is selected. We could have compared any of the five decrements; so that a more realistic probability is $5 \times 0{\cdot}008 = 0{\cdot}04$, since some selection still remains, perhaps even higher.

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FACTOR ANALYSIS, LATENT STRUCTURE ANALYSIS AND MENTAL TYPOLOGY

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The following paper deals with the classification of mentally retarded patients into groups or types, and is based on an analysis of data for over 700 cases. Two alternative methods have been used: first, latent structure analysis (a relatively new procedure suggested by Lazarsfeld) and factor analysis (a standard procedure for classifying persons as well as analysing tests or characteristics). It is found that the former yields a classification that agrees more closely with that proposed by earlier investigators in this field on the basis of subjective or intuitive judgement.

I. Problem and Methods

In the field of mental retardation, typologies and diagnostic groupings are as numerous as in other areas of clinical psychology. The number of somatic categories is ever increasing; and psychological categories show no signs of diminishing. Categories of mental retardation have been proposed by Sarason [8], and additional categories discussed by Spitz [9], Strauss [10], and others. Since the early work of Patterson and Rundquist [6] it has been apparent that mentally retarded individuals can be divided into at least two well defined groups on the basis of sociological variables [2, 7, 11]; and, regardless of the particular classifications eventually reached, it has been subjectively felt that there are in fact at least two major types: but to demonstrate such a typology empirically requires much more than subjective evidence.

Latent structure analysis, as proposed by Lazarsfeld [5], was developed so that a population could be divided, on the basis of its manifest characteristics, into ' latent groups '. Each separate group would evidence homogeneity with respect to the manifest characteristics ; but different patterns of characteristics would be found between groups. The development of latent structure analysis depends heavily on the mathematical techniques of multiple factor analysis presented by Thurstone and others [12]. Hence one may ask if either of these methods will confirm the existence of a typology that everyone subjectively feels must exist, and if so, are there any differences between the typologies developed by the two methods ?

The clinical records of all first admissions to Pacific State Hospital during the fiscal years 1947–1952 were examined for information concerning their families, early childhood characteristics, and their present adjustment and capabilities. This information was then punched on IBM cards for the purposes

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of tabulation. Training of the clerks proceeded to the point that two clerks would agree on four records out of five with respect to every item used in the analysis.

II. Results

Seven hundred and nine patients were admitted to Pacific State Hospital during 1948–1952 with the understanding that they would remain in the institution for treatment. Eight variables were selected from the 195 available from the code sheet. A matrix of joint proportions was computed and is presented in Table I. The variables were analysed on the basis of psychologically and medically meaningful categories. A secondary consideration was given to the distribution of the variables so that no dichotomy would be greater than 70 : 30. One of the two resulting categories was arbitrarily designated as a positive value for the variable.

The matrix of intercorrelations of the eight variables is shown in Table II. A latent structure analysis was performed on the joint proportion matrix, and the data rotated according to the suggestions of Gibson [3]. The rotated loadings are shown in Table III. A factor analysis was carried out with the same data, based on the appropriate correlation coefficients and a complete centroid procedure as described by Thurstone [12]. The factors were rotated to simple structure. The rotated factor loadings are given in Table IV.

Table III reveals that four classes emerge from the latent structure analysis: (in this table, the column at the extreme right indicates the combination of positive responses finally selected for rotation). It might be mentioned that the proper designation of responses as positive facilitates the ease with which rotation may be performed. Since probabilities cannot be greater than 1 or negative, the rotational restriction is often severe [3]. Some combinations of positive responses prove difficult to rotate. Fortunately the unrotated matrix may be altered so as to rearrange positive designations without the necessity of re-factoring [4]

The first latent class is characterized by severely retarded Caucasian children with low IQ's and a somatic diagnosis, whose parents are of a higher socio-economic status. This class corresponds to the severely retarded young group discussed by other authors. The other three classes consist of persons above nine years of age of varying types with higher IQ's. Class II seems to consist of delinquent non-Caucasians, Class III might be characterized as Caucasians with a diagnosis of undifferentiated or familial disorder with high IQ's, and Class IV as older outgoing individuals with high IQ's. It is apparent that Classes II, III, and IV are similar, and might be grouped together into an older, high IQ, delinquent class. Such a grouping would conform to the second major type suggested by various authors.

The factor analysis results were rotated to simple structure, the usual criterion for factor analytic rotations. It must be remembered in interpreting the results of the two methods that a high positive correlation is similar to a high

Table I. Matrix of Joint Proportions

	0	IQ	Release	Race	Diagnosis	Occupation	Delinquency	Direction of Personality	Age	Positive Responses
0		0·391	0·369	0·757	0·564	0·529	0·515	0·589	0·606	
IQ	0·391		0·233	0·233	0·105	0·162	0·202	0·305	0·333	50+
Release	0·369	0·233		0·203	0·111	0·146	0·115	0·285	0·301	Yes
Race	0·757	0·223	0·203		0·492	0·491	0·436	0·412	0·404	White
Diagnosis	0·564	0·105	0·111	0·492		0·360	0·376	0·251	0·250	Medical
Occupation	0·529	0·162	0·146	0·491	0·360		0·332	0·278	0·269	Skilled and above
Delinquency	0·515	0·202	0·115	0·436	0·376	0·332		0·240	0·240	No
Direction of personality	0·589	0·305	0·285	0·412	0·251	0·278	0·240		0·457	Outgoing
Age	0·606	0·333	0·301	0·404	0·250	0·269	0·240	0·457		Over 9

Table II. Matrix of Correlation Coefficients

	IQ	Release	Race	Diagnosis	Occupation	Delinquency	Direction of Personality	Age	Positive Responses
IQ		0·57	0·25	0·44	0·20	0·55	0·40	0·75	50+
Release	0·57		0·31	0·39	0·25	0·42	0·32	0·44	Yes
Race	0·25	0·31		0·30	0·40	0·26	0·19	0·28	White
Diagnosis	0·44	0·39	0·30		0·26	0·39	0·27	0·42	Medical
Occupation	0·20	0·25	0·40	0·26		0·45	0·35	0·30	Skilled and above
Delinquency	0·55	0·42	0·26	0·39	0·45		0·33	0·50	No
Direction of personality	0·40	0·32	0·19	0·27	0·35	0·33		0·51	Outgoing
Age	0·75	0·44	0·28	0·42	0·30	0·50	0·51		Over 9

Note.—The correlation between Race and Diagnosis is the Phi-Coefficient ; the other correlations for these two variables are Point Biserial Coefficients. The remaining correlations are Pearson Product Moment Coefficients.

latent class proportion, but a latent class proportion of 0·50 is similar to a factor loading of zero and a latent class proportion of zero implies a high negative correlation. Comparing the results, Factor I is similar to latent Class I and Factor II is somewhat like latent Classes II and IV, but further identification is impossible.

It is interesting that the factor that gives the greatest loading on release rate gives the highest loading on IQ, where, in the latent structure analysis, the highest release rate was found in a group with rather low IQ's. Another anomalous finding is in rotated Factor II, where we find that non-delinquency and high socio-economic status are loaded on a factor where the loading for Caucasian is only 0·25. This is inconsistent with the latent structure analysis but not with the results published by Sabagh wherein he examines the same data according to incidence rates.

Table III. Rotated Group Structure

Variable	Class I	Class II	Class III	Class IV	Positive Responses
0	1·00	1·00	1·00	1·00	
IQ	0·06	0·40	0·85	0·89	50+
Release	0·92	0·26	0·54	0·35	No
Race	0·01	0·81	0·12	0·34	Non-white
Diagnosis	0·91	0·24	0·00	0·33	Medical
Occupation	0·27	0·99	0·65	0·42	Low
Delinquency	0·74	0·72	0·29	0·54	None
Direction of personality	0·68	0·30	0·18	0·02	Withdrawn
Age	0·71	0·20	0·09	0·01	Under 10
Proportion in each class	0·49	0·18	0·09	0·23	

Table IV. Rotated Factor Loadings

	I	II	III	IV	Positive Responses
IQ	−0·85	0·14	0·24	0·08	50+
Release	0·62	−0·11	−0·09	−0·30	No
Race	−0·13	0·25	0·20	0·50	Non-white
Diagnosis	0·44	−0·18	−0·09	−0·36	Medical
Occupation	−0·02	0·56	0·30	0·37	Medical
Delinquency	0·51	−0·61	−0·05	−0·15	None
Direction of personality	0·33	−0·20	−0·52	−0·10	withdrawn
Age	0·65	−0·23	−0·53	−0·07	Under 10

III. A Comparison of the Two Methods

Latent structure analysis, a method for dividing a population into categories based on manifest characteristics, appears essentially to substantiate the existence of a typology which is well recognized in the field of mental retardation. A factor analysis of the same data does not seem to provide such a simple and coherent

picture. One reason for this finding could well be associated with assumptions of the two techniques as discussed by Gibson [3], Green [4], and Lazarsfeld [5]. The latent structure equations do not imply any assumptions concerning dimensionality or linearity. It is simply a method for extracting ' latent classes ' within which responses to items are independent in the coin-tossing sense. Factor analysis, which is based on correlations, implies linearity between tests and factors, and is concerned directly with dimensionality. If all the assumptions of factor analysis were met, the correspondence between the models would be very close (Gibson [3]); but differences arise when the necessary conditions are not fully met. In addition, it is also possible that a different rotational solution would produce greater clarity in the factor structure. The solution given essentially followed the common rules of factor analytic rotation, and did not attempt to rotate to maximum agreement between the two methods.

A second hypothesis is that the latent class analysis is less sensitive to variations within groups, and would therefore tend to produce gross typologies. Furthermore there is the possibility that the factor analysis reflects more of the variation within sub-classes and that the latent structure results reflect groups of patients within broad categories.

A final comment must take note of the type of correlations involved in the factor analysis. An examination of the variables demonstrates that three types of correlations (Pearsonian r, point biserials, and phi) are present in Table III, the matrix of intercorrelations. The effect of this mixing is unknown; but in view of the discussion in Dingman [1] this should not be discounted.

IV. Conclusions

A relatively new procedure proposed by Lazarsfeld seems to produce a typology that is subjectively accurate as judged by the views of numerous investigators in the field of mental retardation. A similar technique in common use for the same purpose is factor analysis. This makes more assumptions; but, although the two statistical procedures are closely analogous, factor analysis here yields a picture which is not entirely consistent with that presented by the latent structure analysis. No doubt factor analysis and latent structure analysis have each its own particular rôle in typological investigations; but only further research can indicate under what specific conditions either is the more appropriate.

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Vol. XIV Part 1 The British Journal of Statistical Psychology May 1961

THE ROTATION OF PRINCIPAL COMPONENTS

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Doubt has been expressed as to the validity and desirability of rotating principal components. In the present case a battery of tests, which was expected to give clear-cut factors, was analysed by the method of principal components, and the components were rotated orthogonally. It is shown that the results thus obtained were in keeping with those that were expected. It is concluded that, whatever may be the weaknesses of rotation as a technique, it is one which can be applied to principal components quite as well as to factors obtained by other procedures.

I. The Problem of Rotation

With the increasing availability of electronic computors the method of factor analysis known as principal components[1] has now become a feasible undertaking. Already a number of researches have appeared in which this technique has been used with sizable batteries of tests. There seems to be a reluctance, however, to rotate the loadings obtained, and in some quarters a doubt as to whether with this procedure rotation is legitimate. The purpose of this paper is to present a set of data experimentally obtained where it was possible to postulate in advance what factors were to be expected, and to show how far the same factors were arrived at by a principal components analysis followed by an orthogonal rotation.

An analysis into principal components (or, as it is sometimes termed, ' weighted summation ') can, like a centroid analysis (the method of ' simple summation '), be carried out with either unities or communalities in the diagonal cells. Which device is adopted depends on whether the factorist is chiefly concerned to relate factors and factor scores to the original test scores, or whether he wishes to describe the battery of inter-correlations in terms of the smallest number of common factors. In a centroid analysis the general practice is to put communalities (or their estimates) into the leading diagonal, though this is not an indispensable feature of the method. With a battery much larger than our own the choice would make little difference to the outcome ; certainly it cannot in itself provide a criterion for the application or non-application of subsequent rotational procedures. In an analysis by principal components it is more usual to take unities as the diagonal entries ; indeed we must do if we

[1] It is unnecessary to argue here whether or not the results of an analysis by principal components may legitimately be termed ' factors '. The origin of the terminology and the problems so raised have been fully discussed elsewhere by Burt (1940), Kendall (1957), Eysenck (1953), and others. In what follows the terms ' component ' and ' factor ' will be used interchangeably.

want the factor scores to be calculated so that the test scores can be reconstructed from them *exactly*; but again this practice is not an essential feature of the method. Evidently then the decision whether rotation is or is not permissible cannot depend on grounds such as these. What are the reasons for or against using unities in various types of investigation has been discussed at length in an earlier issue of this *Journal* [1, 9]. Since our aim was to offer a demonstration that would be generally acceptable, we ourselves decided to keep to the usual practice.

The special advantage of using principal components is that each factor accounts for the greatest amount of the total test variance remaining before it is extracted. If unities are used for the self-correlations, then the number of factors required will be exactly equal to the number of tests in the battery (not greater as with the centroid method where specifics are also postulated). These features give the method a strong appeal to the mathematician. Indeed, most mathematicians go further, and apparently hold that, since the maximum amount of the residual variance is accounted for at each stage of factorization, it would be pointless to rotate to positions where this feature was lost. An analogous argument used to be put forward in the case of the centroid procedure : it was said that, since each axis passes through a particular point—a centroid—it would be a pity to lose this feature by subsequent rotation, and that the bipolar factors which it yields give an objective and dichotomous classification based directly on the data which the subsequent rotations are apt to obscure. However, in all such matter, as has been emphasized by Burt [3], one ought to be guided, not merely by mathematical, but also by psychological considerations. Psychologically neither the centroid nor the maximum variance features have any special virtue ; they may be considered to be in much the same position as the various transformations which can be used before analysis of variance techniques are applied ; that is to say, they are interim devices which facilitate certain subsequent mathematical manipulations.

In an instructive discussion on factor analytic methods [6] Maxwell mentions one feature in particular as contra-indicative of the rotation of principal components, namely, the poor fit which principal components give to the original correlation matrix when the number of factors extracted is fewer than the number of tests. And indeed his objections are frequently quoted by critics when one proposes to supplement an analysis into principal components by an orthogonal rotation to group factors or the like. Now, as we have already stated, with p tests p components are required to reproduce the original correlations exactly ; and we would agree that in many cases, if not all, each of the p components ought, if possible, to be computed. But we would suggest that the inability of a limited number of principal components (say the first k) to reproduce the observed correlations exactly is a defect resulting from the limitation in the procedure, not a special weakness to be associated with the subsequent rotations. After all with principal components we have a valid test of significance due to Bartlett [2], which will guide us in considering when to stop

extracting further factors. Often the data obtained are not sufficient to render *all* the factors statistically significant. In such a case a limited number of principal components may give almost as much information as all of them, in regard both to the correlations and the factor scores of the individuals tested (see Thomson [8], p. 117). We think therefore, that a limited number of principal components may be useful, but that this issue has no particular bearing on the rotation problem.

In our opinion the decision whether or not to rotate must turn mainly on one's view of the rotational procedure in itself and not on whether it has been preceded by a principal component, a centroid, or any other well-established method. If one believes that rotation allows of a better or easier psychological interpretation, then, we submit, it can be legitimately used no matter what method of factor analysis has preceded it. The arguments in favour of rotation are not mathematical; and in each research the investigator has to decide its merits on non-mathematical grounds.

II. An Illustrative Experiment

Our experiment [7] was concerned with measuring the attitudes of 200 men and women training college students towards the people of four other national or racial groups, viz., American (defined as the white inhabitants of U.S.A.), Chinese, Negroes, and Russians. For each of these the respondents were asked to check a number of items or questions—the same for every group. The questions were presented in three forms—as a Bogardus social distance test, as a Thurstone and Chave attitude test, and as a Likert attitude test. There were thus 12 tests in all.

From the intercorrelations of the tests seven principal components were calculated[1], unities being used as diagonal entries. The correlation table and final residuals are shown in Table I, and the components and their contribution to the variance in Table II. At the time when the analysis was undertaken there was a limit to the number of components which could be taken out in a reasonable period by the machine available ; the decision to stop at seven factors was due partly to this and partly to the small amount of variance remaining to be accounted for at this stage. Bartlett's test showed that all seven components were fully significant, and the trend of the values for chi-squared calculated after each factor had been extracted suggested that one or two significant factors might still remain.

Orthogonal rotations to secure a positive manifold and a maximum number of zero or near-zero loadings were carried out by means of a two-by-two rotation. A modification of Zimmerman's method [10] was used—the new loadings being calculated arithmetically instead of being merely measured on the graph. Rotation to oblique positions might have resulted in a slightly better fit to the

[1] We are indebted to Mr. W. L. B. Nixon of the University of London Computor Unit for help in programming and carrying out this task.

Table I. Observed Correlations and Residuals

Test	1	2	3	4	5	6	7	8	9	10	11	12
1		0·318	0·283	0·159	0·119	0·071	0·088	0·034	0·423	0·206	0·229	0·172
2	−0·032		0·777	0·625	0·177	0·468	0·379	0·433	0·297	0·722	0·633	0·565
3	−0·018	−0·057		0·530	0·056	0·367	0·388	0·315	0·250	0·631	0·733	0·494
4	−0·019	−0·011	−0·038		−0·006	0·224	0·114	0·521	0·172	0·458	0·407	0·731
5	0·029	0·035	0·005	−0·017		0·307	0·378	0·229	0·317	0·194	0·156	0·114
6	−0·011	−0·034	0·021	0·055	−0·002		0·572	0·528	0·196	0·557	0·416	0·409
7	−0·013	0·032	−0·045	0·039	−0·012	0·006		0·339	0·134	0·372	0·400	0·201
8	−0·002	0·018	0·048	−0·081	−0·002	−0·060	−0·046		0·096	0·390	0·337	0·625
9	−0·059	0·050	0·024	0·043	−0·038	0·022	0·021	0·010		0·498	0·595	0·330
10	0·028	−0·005	−0·053	0·004	0·008	−0·041	0·019	−0·002	−0·051		0·842	0·648
11	0·035	−0·039	0·004	−0·035	0·027	−0·009	−0·042	0·039	0·022	−0·008		0·612
12	0·025	−0·020	−0·004	−0·038	0·023	0·013	0·006	−0·076	−0·057	0·000	−0·002	

TABLE II. PRINCIPAL COMPONENTS

Type of Test	National Group	I	II	III	IV	V	VI	VII	h^2
Bogardus	1. American	0·334	−0·004	0·657	0·176	0·590	−0·215	0·012	0·969
	2. Chinese	0·844	−0·177	0·022	−0·188	0·194	0·118	−0·247	0·892
	3. Negro	0·785	−0·200	0·085	−0·383	0·150	0·227	−0·025	0·884
	4. Russian	0·668	−0·499	−0·199	0·277	0·135	0·212	0·045	0·877
Thurstone and Chave ..	5. American	0·289	0·725	0·060	0·373	0·000	0·453	−0·171	0·986
	6. Chinese	0·646	0·424	−0·326	−0·082	0·023	−0·399	−0·268	0·942
	7. Negro	0·527	0·578	−0·218	−0·317	0·220	0·039	0·394	0·965
	8. Russian	0·622	0·027	−0·518	0·396	0·110	−0·152	0·092	0·857
Likert	9. American	0·496	0·188	0·641	0·281	−0·302	−0·108	0·113	0·887
	10. Chinese	0·868	−0·003	0·114	−0·142	−0·303	−0·087	−0·135	0·904
	11. Negro	0·836	−0·034	0·201	−0·235	−0·295	0·025	0·139	0·903
	12. Russian	0·787	−0·286	−0·161	0·334	−0·139	−0·050	0·127	0·876
Contribution to Variance (per cent)		44·86	12·31	11·6	8·0	6·5	4·7	3·3	91·2

expected results (some of the negative loadings would probably have disappeared); but orthogonality was retained on account of its simplicity.

III. The Factors Expected and the Factors Obtained

The nature and number of the tests and the nationalities led us to expect factors of four main types. Owing to the limited number actually extracted not all had room to appear. But we postulated that we might get the following.

(i) A general factor indicating the respondent's general attitude to all foreign nationals no matter what type of test was being used (since rotation of the kind we employed usually eliminates the general factor we regarded this merely as a possible but not as a probable factor) ;

(ii) Four group factors corresponding to the attitudes towards each of the four national groups ;

(iii) Three group factors corresponding to each of the three types of test used (we expected greater similarity between the results of different tests than between the results for different nationalities) ;

(iv) Group factors linking together (*a*) white groups and (*b*) non-white groups respectively. These again we regarded as possible factors only.

The foregoing scheme of factors is shown diagrammatically in Table III.

Table III. Expected Distribution of Group Factors

	Factors									
Test	I	II	III	IV	V	VI	VII	VIII	IX	X
A.1	+	+				+			+	
C.2	+		+			+				+
N.3	+			+		+				+
R.4	+				+	+			+	
A.5	+	+					+		+	
C.6	+		+				+			+
N.7	+			+			+			+
R.8	+				+		+		+	
A.9	+	+						+	+	
C.10	+		+					+		+
N.11	+			+				+		+
R.12	+				+			+	+	

The factors actually obtained are set out in Table IV. So far as possible, they are set out in the same order as those in Table III. The heaviest loadings are printed in semi-bold type. In some cases, however, there are other large factor loadings which tend to detract from the simplicity of the picture ; but they are always smaller than the semi-bold figures which appear *in the same column.*

It will be seen that

(i) there is no general factor ;

TABLE IV. ROTATED COMPONENTS

Test	I	II	III	IV	V	VI	VII	h^2
A.1	**0·635**	0·276	0·219	−0·073	**0·499**	−0·137	−0·408	0·967
C.2	0·055	**0·526**	0·293	0·455	**0·481**	0·237	0·171	0·889
N.3	−0·023	0·418	**0·540**	0·341	**0·452**	0·156	0·267	0·883
R.4	−0·051	−0·011	0·155	**0·785**	**0·480**	0·056	0·011	0·877
A.5	**0·614**	−0·169	−0·210	−0·013	0·111	**0·590**	0·422	0·988
C.6	0·071	**0·540**	0·028	0·236	−0·116	**0·745**	−0·142	0·942
N.7	0·066	0·005	**0·583**	−0·112	0·068	**0·779**	0·014	0·968
R.8	−0·003	−0·047	0·003	**0·647**	0·116	**0·581**	−0·290	0·856
A.9	**0·800**	0·167	0·294	0·290	−0·197	−0·084	0·070	0·889
C.10	0·223	**0·545**	0·374	0·552	−0·061	0·231	0·231	0·902
N.11	0·218	0·374	**0·624**	0·479	−0·043	0·150	0·267	0·902
R.12	0·125	0·069	0·228	**0·868**	−0·124	0·185	−0·046	0·878
Contribution to Variance (per cent)	12·9	11·0	12·5	23·4	8·5	17·2	5·7	91·2

(ii) the first four rotated factors correspond quite well with the four factors postulated above, indicating attitudes towards the four different national groups ;

(iii) of the three group factors postulated to correspond with the types of test employed, two appear quite clearly, namely, factors V and VI, but the group factor corresponding to the Likert tests does not appear (there is in V, however, a sign pattern which suggests that the Likert tests might separate out if more factors had been available for rotation) ;

(iv) the last factor, factor VII, is apparently meaningless; but it is by far the smallest in the seven ; there is, however, a suggestion in the figures that they may serve to distinguish between the American Bogardus test (A.1) and the American Thurstone-and-Chave test (A.5) ;

(v) the remaining factors postulated in Table II, namely factors IX and X, have not appeared.

IV. CONCLUSION

It is not our purpose to discuss in any detail the results obtained from the point of view either of the experimental procedure or of the attitudes revealed. Here our concern has been focused solely on a single point of methodology ; and our conclusion is that the pattern of loadings finally reached is sufficiently close to that which was postulated at the outset to demonstrate beyond question that the rotation of principal components is quite as permissible and reasonable a procedure as the rotation of factors derived by any other procedure.

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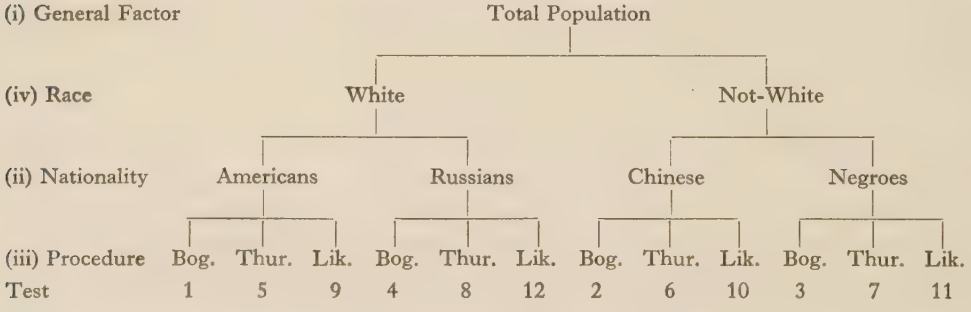
APPENDIX[1]

A Note on Factor Patterns

By Cyril Burt

The Block-and-Line Pattern. The illustrative example, described in Dr. Burroughs and Mr. Miller's paper, is of special interest to the factorist. Factors are essentially principles of classification ; and the authors' full classificatory scheme furnishes an admirable instance of what may be called *subdivided factors* with *cross classification.* Comparatively few writers recognize how frequently ' subdivided factors ' occur in mental testing; and, even when they recognize them, they usually assume the classification is a ' divergent ' classification, never wholly or partly a ' cross-classification '.[2]

The divisions and subdivisions postulated by the authors involve four *fundamenta divisions,* as the logicians would say—four ' principles of classification ', entailing *one* general factor, *two* racial factors, *three* procedural factors, and *four* nationality factors—ten factors in all to be calculated from a battery with twelve tests (Table III. As formulated by the authors in their preliminary specification of the ' expected factors ' (p. 48) their complete scheme may be tabulated as follows :



However, they consider the first two sets of factors mentioned above—the general factor and the racial factors—not as probable but only as possible factors ; and these factors do

[1] As will be seen, the following note arose out of certain criticisms made by the referee and quoted on p. 44 below. I am grateful to Dr. Burroughs for his kindness in reading through an earlier draft, and elucidating one or two points about which I was in doubt.

[2] For the terminology, which is borrowed from the logicians' discussion of the theory of classification, see this *Journal,* II, 1949, p. 46.

not appear in their rotated factor matrix (Table IV). We are thus left with a simple factor-scheme of great importance which occurs again and again in mental testing, namely, one in which each of the tests can first be classified according to one particular characteristic—most frequently its *content*—and then cross-classified according to a second characteristic, usually the procedure or *form*. As a rule, the effect of this is at once obvious in the correlation table : one set of factors (here the factors distinguishing the tests according to their procedure) produces blocks of high correlations astride the leading diagonal, while the other set produces one or more oblique rows of high correlations parallel with the diagonal (see Table I above). Among the factor studies previously published in this *Journal* perhaps the clearest instance of this scheme is to be found in A. F. Mahmoud's investigation (VIII, 1955, pp. 121f., Tables II and VI): it relates to eight tests only instead of twelve; but, as we shall see, his correlation table exhibits the same ' block-and-line ' scheme as Table I.

Possible Criticisms. The immediate purpose of Dr. Burroughs and his colleague, however, was to demonstrate that the factors obtained by weighted summation (' principal components ' as they are sometimes called) can be validly rotated so as to obtain group factors in place of the bipolar factors, much as the factors obtained by simple summation are often rotated. There can, I think, be little question that the results they have obtained fully refute those writers who maintain the more extreme view that such a rotation is in no circumstances justifiable. The authors quote Dr. Maxwell, who mentions " one feature in particular as contra-indicative of the rotation of principal components, namely, the poor fit which principal components give to the original correlation matrix when the number of factors extracted is fewer than the number of tests ". There seems to be a good deal of confusion here. Table I (p. 38) contains 66 observed coefficients obtained by correlating 12 tests. Now with only eight factors we can obtain a *perfect* fit with either principal components (weighted summation) or the centroid method (simple summation), provided suitable communalities are inserted. With only seven factors (as in Table IV above) the fit would not be so good, but it would certainly be better with principal components than with any other method. But possibly, when Dr. Maxwell speaks of ' principal components ', he tacitly assumes that unities not communalities are inserted in the leading diagonal. In that case the fit for 8 factors will undoubtedly be poorer than with communalities. But it will be still decidedly closer than with simple summation, since the method of principal components minimizes the residuals.[1]

However, as has already been pointed out by one critic in correspondence, the analysis carried out by our two authors still leaves a loophole for the more moderate opponents

[1] In an early issue of this *Journal* (II, 1949, pp. 90f., ' The Alternative Methods of Factor Analysis and their Relations to Pearson's Method of Principal Components') we reproduced the original correlation matrix for which Pearson proposed the method of principal components. A complete factor analysis was carried out by that method, first with unities in the leading diagonal (yielding seven factors for seven ' tests '), then with estimated communalities (yielding four genuine factors, with a negligible fifth to account for residuals). Figures obtained with simple summation are tabulated for comparison ; and the reader can see that the fit is much better with principal components. The results of an orthogonal rotation are also given.

Whether unities or communalities are inserted for the preliminary analysis makes no difference if the analysis is complete 'or virtually complete' so that the discrepancies are non-significant. In either case the same group factor matrix can be obtained by rotation, for, when unities are used, we have only to add a diagonal set of specific factors to bring the communalities up to unity : moreover, the rotation is still orthogonal. Thus, since the initial factor matrix is to be rotated in any case, it makes little difference how it is obtained, as the same group factor matrix can be reached from either type of bipolar matrix. If an electronic computor is available then it is simpler to work with principal components, since nearly all such machines are programmed to carry out this type of matrix-transformation ; if not, simple summation (the ' centroid ' method) will yield the same rotated factors, and at the same time be far quicker for those who only use a desk machine.

of such rotations. " Dr. Burroughs and Mr. Miller ", he writes, " were already aware of the nature and possible resemblances of the several tests and the most likely way in which they would be grouped ; and, as has been frequently shown, when one is armed with this knowledge, it becomes all too easy to rotate the bipolar matrix so as to secure a plausible confirmation of almost any hypothesis one prefers. The task is particularly simple if one is content to end up with one or two meaningless factors (like factor VII in Table IV), and with one or two negative loadings (like those for test A1, test A5, and test R8) to eke out any deficiencies in the fit ; and, when there is a large general factor in the unrotated matrix and none in the hypothetical, there should be no difficulty whatever : for then one can use the general factor loadings to fill out almost any group factors one wishes, and the superfluous overlap can be transferred to other factors. In the present case the numerous positive loadings left after rotation in factors III and IV (which are almost general factors in themselves) suggest that something of this sort has happened ; and as a further result at least one rotated factor and a good many factor loadings appear which were not expected or accounted for in the hypothesis proposed. There is too a marked overlapping in the case of the last four tests, the four loadings in each of the seven factors being (with two minor exceptions) of the same sign in every case : all four tests are of the Likert type, so possibly this indicates a ' Likert factor ' which, as the investigators observe, has not been analysed out ".

The Need for Blind Rotation. I should not myself agree with all these criticisms. To begin with, I think, the investigators have made the aim of confirmation somewhat harder by deciding to dispense with a general factor in the rotated matrix : had they retained it the demonstration would have seemed far more convincing, and many of the meaningless positive loadings in the group factors would have disappeared or been greatly diminished. Secondly, the essential pattern is still obscured by the presence of specific factors which were introduced with the unities and have not been removed by rotation. The fact that most of the observed correlations are well below unity makes it certain that each test must contain a fairly large specific factor ; and by putting unities in the leading diagonal the investigators are forced to convert these to terms of bipolar factors. To secure a distinctive sign pattern for each of the 12 tests help from at least four bipolar factors is needed ; and the modified loadings this produces may well account for the fact that some of the factors and factor loadings seem almost meaningless.

However, the ideal way of meeting the foregoing criticisms would be to ask an independent factorist to carry out a blind rotation. He would then be compelled to base his rotation on the empirical data in front of him, not on his knowledge of the tests or of the particular hypothesis he wished to prove.

The Bipolar Factor Matrix as a Guide. In attempting such a rotation the factorist would, I imagine, find himself obliged to follow the same principle as that adopted by Mahmoud in the article already cited; he would determine the ' grouping ' of the ' group-factors ' by examining the sign pattern of the bipolar matrix (cf. *loc. cit. sup.*, p. 134 ; the full procedure is described in this *Journal*, III, pp. 53f.). Now, with one or two important exceptions, the whole of the authors' postulated factors can be inferred—so far as the structural pattern is concerned—from the set of unrotated factors shown in Table II.

But, before we attempt to interpret this table, let us recall that, as a general rule, each bipolar factor will classify n tests into two groups with $n/2$ tests in each ; when the rotated factor matrix contains groups with smaller numbers in them, then in the unrotated factor matrix at least two bipolar factors will be needed to express them. Let us now begin with the largest bipolar in Table II (factor II). From what has just been said it must clearly be interpreted in relation to factor V. Together they subdivide the battery into three groups of four tests each. These, as we can see (though our blind factorist cannot), correspond with the three main types of test *procedure* : (factor V contrasts the Likert tests with the other eight, and factor II then subdivides the latter into the Bogardus type and the Thurstone-and-Chave type).

The next largest bipolar is factor III. This must evidently be interpreted in relation to factor IV. Tests 1, 5 and 9 have positive loadings in both. Tests 4, 8 and 12 have positive loadings in factor IV, but negative loadings in factor III. This gives two further groups, each containing three tests. (Knowing the content of the tests, we can identify those just mentioned as tests relating to the Americans and to the Russians respectively.) Factors VI and VII distinguish between the Chinese and the Negroes, except in the Bogardus group, where there seems to be no evidence for such a distinction: (in this case I find it difficult to imagine any factorist introducing such a distinction as indicated by the loadings in bold type in Table IV, unless he already knew that his hypothesis required it; this is surely a point where a blind factorist would *not* verify the expected scheme).

Dr. Burroughs and Mr. Miller apparently found no indication of the group factors linking together (*a*) white groups and (*b*) non-white groups (factors IX and X). But factor IV in the bipolar matrix appears to do precisely this.

They are also puzzled, so they tell us, by the nature of their last and smallest factor—factor VII. In this only two loadings are of any appreciable size—one negative (−0·408 for test A1) and one positive (+0·422 for test A5). This produces a negative correlation between tests A1 and A5. Now the first factor of all—factor I—has exceptionally large loadings for the three 'American' tests—A1, A5, and A9. This is in keeping with the table of *expected* factors. But there is nothing either in the *observed* correlations or in the unrotated factors to support so large a group factor. The correlation between A1 and A5 is only 0·119 ; and the other two correlations are not much larger. Clearly factor VII with its big negative loading is required merely to cancel the effect of the two large positive loadings assumed for factor I. The negative loadings for A5 in factor II and for A9 in factor V have a similar purpose. Had the three loadings in factor I been smaller there would have been no need to introduce these negative loadings.

The Correlation Table as a Guide. The expected distribution of the group factors shown in Table II happens to be of a type which, when present, commonly shows up fairly distinctly in the matrix of observed correlations. Let us look once again at the correlation table and accompanying factor matrix printed in Mahmoud's paper. Except that there are only 2×4 tests instead of 3×4, the factor pattern (*loc. cit.*, p. 133, Table VI) is almost identical with that shown in Table III above. The 'basic factor' in the former corresponds with factor I in the latter; the next two factors (group factors with four loadings, representing different types of test content) correspond with factors VI and VII ; the last four ("quasi-specifics peculiar to different *forms* of test", with loadings arranged obliquely) correspond, as the scheme is now arranged, with factors II, III, IV, and V, and the two remaining factors (for the 1st and 2nd days) correspond with factors IX and X. Naturally there is nothing answering to factor VIII, since this represents a *third* application of the four subtests redesigned to conform with a third type of test procedure (in the present case, the Likert).

When we turn to the two correlation matrices, the correspondence is almost as striking. In both tables the pattern of high coefficients produced by the pattern of factor loadings stands out quite clearly. Indeed it is, if anything, still more conspicuous in Table I above (p. 38) because of its larger size. First, factors II, III, IV, and V (p. 40) leave two oblique ridges of high correlations, parallel to the high self-correlations ordinarily inserted in the leading diagonal, and running from the correlation between tests 1 and 5 down to that between tests 8 and 12 and again from the correlations between tests 1 and 9 to that between tests 4 and 12. Secondly, factors VI, VII, and VIII each produce a 4×4 square of high correlations astride the leading diagonal. In addition to this factor X is responsible for the large isolated correlations between the tests relating to the Chinese and the Negroes (tests 2 and 3, 6 and 7, 10 and 11, etc.). Factor IX, according to the hypothesis proposed, ought to have the same effect on the tests relating to the Americans and Russians ; but it does not.

Discrepancies between Data and Hypothesis. The popularity of the ' hypothetico-deductive ' theory of scientific proof has led many research students to suppose that the mark of a successful experiment is to find empirical data agreeing in detail with the hypothesis which the investigator wishes to establish. But the really instructive features of an inquiry are more likely to be those unexpected discoveries which were not contemplated in, or deducible from, the initial hypotheses. It is from the discrepancies which seem to correct or contradict our preconceived notions that we learn most.

In the present investigation the expected classification appears to be contradicted at two significant points. First, we find that, when the two ' white nations ' are American and Russian, the expected group ' factor linking white groups ' does not appear. Secondly, we note that the correlation between the attitudes towards the Chinese and the Russians is almost as high as the correlation between the attitudes towards the Chinese and the Negroes. What inferences are to be drawn from these two discrepancies can hardly be decided without reference to the actual questions that were asked. It may be that some popular antithesis, like that between ' communist ' countries and ' non-communist ' or between groups with a ' Western ' way of life and a ' non-Western ' way has been more influential in determining the students' attitude than any simple distinction according to colour or race. But that is a problem which could hardly be solved by further statistical analysis of the data that have been given.

Computing Methods. Whenever the battery of tests is large and the factor pattern therefore somewhat intricate, most factorists, it would seem, find it difficult to trace the clues to the arrangement of the group matrix in the initial bipolar matrix. It may therefore be helpful to examine an illustrative instance of the pattern with which we have here been concerned, reduced to its simplest possible terms. For purposes of demonstration it is usually best to take *exact* figures for the hypothetical scheme and to assume that the loadings in the same factor are always equal. The relations then stand out unobscured, and the accuracy of the rotation can be judged beyond question.

1. *Bipolar Analysis.* Table I A gives the smallest possible correlation table, which will show the same type of pattern as that displayed in Table I on p. 38. The ' blocks ' of augmented coefficients are printed in semi-bold, and the oblique ' lines ' in italic. A complete analysis into principal components has been carried out, and the resulting factor loadings are shown in Table II A. The sign pattern in the table can be compared with that set out in Table II above.

The scheme of classification which the factor pattern implies is here unmistakable. We have (i) a general or ' basic ' factor common to all four tests; (ii) two fairly large group factors common to (*a*) tests 1 and 2, and (*b*) tests 3 and 4; (iii) two slightly smaller group factors common to (*a*) tests 1 and 3, and (*b*) tests 2 and 4; (iv) finally the fact that there is a unique pattern of signs for each of the four tests indicates that each has its own specific. All this we might have inferred by inspecting the correlation table, though with less confidence.

2. *Group Factor Analysis.* To determine the matrix of group factors we begin by picking out correlations between tests which, according to our scheme, could be due only to the single general factor, e.g., the correlation between tests 1 and 4. With no more than one pair of tests we must assume equal factor loadings, viz., $\sqrt{(0{\cdot}500)} = 0{\cdot}707$. For the other factors we have $\sqrt{(0{\cdot}750 - 0{\cdot}707^2)} = 0{\cdot}500$, $\sqrt{(0{\cdot}660 - 0{\cdot}707^2)} = 0{\cdot}400$, and $\sqrt{(1{\cdot}000 - 0{\cdot}707^2\ 0{\cdot}500^2 - 0{\cdot}400^2)} = 0{\cdot}300$. The complete results are shown in Table IV A.

We could proceed in exactly the same way with Table I in the preceding paper. It is plain, for instance, that, if we accept the hypothetical scheme shown in Table III, then the low correlations of test 1 with tests 6, 7, 10, and 11 must be due to the general factor only. With this large battery we have for each test as many as *four* correlations due solely to this one factor ; we must therefore proceed by averaging (see this *Journal*, III, pp. 47f., for details). After subtracting the effect of the general factor we next calculate the loadings for factors IX and X. For this we have two correlations for each test (e.g., those of test 1

with 8 and 12). Next we can calculate the loadings for the three main blocks (factors VI, VII, and VIII) and finally those for factors II, III, IV, and V.

3. *Rotation.* Returning to our artificial example we must now attempt to reconstruct the original correlations from the new group factor matrix, and calculate the various residuals. If (as will probably be the case with an empirical correlation table) some of the residuals are fully significant and thus imply some measure of overlap, we must next proceed to a formal rotation. The requisite rotation matrix is given in Table III A. Calculation will show that the sum of squares for each row is 1·000 and the product-sum for each pair of rows is zero : in other words, the table consists of the first four rows of a 9×9 orthogonal matrix. On multiplying the factor matrix for principal components by this rotation matrix we obtain the loadings set out in Table IV A. Our hypothetical scheme is thus confirmed; and, since the correlation table (Table I A) was in fact constructed from Table IV A, the accuracy of the results demonstrates the validity of applying orthogonal rotation to a factor matrix obtained for principal components.

4. *The Reduced Correlation Matrix.* I now want to show that the same result could have been obtained much more speedily with the more usual procedure. We begin by inserting those particular self-correlations (or ' communalities ') which will reduce the correlation matrix to its lowest possible rank—here a rank of three[1]. The requisite values are 0·91. We then factorize this reduced matrix by simple summation, and obtain a bipolar matrix containing three factors only instead of four (Table II B). This implies the same classificatory scheme as before ; but the rotation matrix will be different. It now contains only five columns instead of nine (Table III B) ; but the square-sums are still unity and the product-sums zero. On postmultiplying Table II B by this orthogonal matrix we obtain the same loadings for the group factors as before, but the specific factor loadings must now be calculated by the formula $\sqrt{(1-h^2)} = \sqrt{(1-0{\cdot}91)} = 0{\cdot}30$.

So far as theoretical considerations go, therefore, it would seem to be almost a matter of indifference whether the rotation starts from a matrix of principal components obtained by weighted summation or from a matrix of centroid factors obtained by simple summation, or again whether we start with unities or with communalities in the leading diagonal. With communalities, the factor pattern is likely to show up rather more clearly in the bipolar matrix. But in the main the choice will, in my view, rest chiefly on practical grounds. For the ordinary research-student equipped only with a desk-machine, factorization by simple summation applied to a table with communalities will prove by far the simplest and quickest procedure, and as accurate as any other method if the results are eventually to be rotated.

[1] In theory, so long as we are free to choose the diagonal entries, a 4×4 matrix should be reducible to rank two. But on solving the requisite determinantal equations we obtain for the diagonal entries 1·4717 (twice) and 0·5083 (twice). This certainly yields two factors only, viz.

I. 0·9929, 0·7100, 0·7100, 0·9929,
and II. 0·6972, 0·0645, −0·0645, −0·6972.

But for a correlation matrix 1·4717 would be an impossible value.

In the illustrative example here chosen the results of simple and weighted summation are the same. But this does not invalidate the argument. If any reader feels a doubt on this point, then he can easily construct a suitable example for himself. He must take *different* values for the test-loadings in the same general or group factor. This will produce a slightly more complex matrix and entail a slightly more complicated set of calculations, and so obscure the points I wished to bring out ; but would give a clearer answer on this further issue, if it were raised.

Table I A. Correlations

Test	i	ii	iii	iv
i	**1·00**	**0·75**	*0·66*	0·50
ii	**0·75**	**1·00**	0·50	*0·66*
iii	*0·66*	0·50	**1·00**	**0·75**
iv	0·50	*0·66*	**0·75**	**1·00**

Table I B. Correlations

Test	i	ii	iii	iv
i	**0·91**	**0·75**	*0·66*	0·50
ii	**0·75**	**0·91**	0·50	*0·66*
iii	*0·66*	0·50	**0·91**	**0·75**
iv	0·50	*0·66*	**0·75**	**0·91**

Table II A. Principal Components

Factor	I	II	III	IV
i	+0·8529	+0·3841	+0·3202	+0·1500
ii	+0·8529	+0·3841	−0·3202	−0·1500
iii	+0·8529	−0·3841	+0·3202	−0·1500
iv	+0·8529	−0·3841	−0·3202	+0·1500

Table II B. Centroid Factors

Factor	I	II	III
i	+0·8396	+0·3535	+0·2828
ii	+0·8396	+0·3535	−0·2828
iii	+0·8396	−0·3535	+0·2828
iv	+0·8396	−0·3535	−0·2828

TABLE III A. ROTATION MATRIX

Factor	G	Gr 1	Gr 2	Gr 3	Gr 4	Sp 1	Sp 2	Sp 3	Sp 4
I	+0·6873	+0·0859	+0·0859	+0·0550	+0·0550	+0·0077	+0·0077	+0·0077	+0·0077
II		+0·4237	−0·4237			+0·0381	+0·0381	−0·0381	−0·0381
III				+0·3903	−0·3903	+0·0549	−0·0549	+0·0549	−0·0549
IV						+0·2500	−0·2500	−0·2500	+0·2500

TABLE III B. ROTATION MATRIX

Factor	G	Gr 1	Gr 2	Gr 3	Gr 4
I	+0·8420	+0·2977	+0·2977	+0·2320	+0·2320
II		+0·7071	−0·7071		
III				+0·7071	−0·7071

TABLE IV A. GROUP FACTOR MATRIX

Factor	G	Gr 1	Gr 2	Gr 3	Gr 4	Sp 1	Sp 2	Sp 3	Sp 4
i	0·7071	0·5000		0·4000		0·3000			
ii	0·7071	0·5000			0·4000		0·3000		
iii	0·7071		0·5000	0·4000				0·3000	
iv	0·7071		0·5000		0·4000				0·3000

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NOTES AND CORRESPONDENCE

FACTOR ANALYSIS AND ITS NEUROLOGICAL BASIS

To the Editor, British Journal of Statistical Psychology

SIR,—In the discussion with Mr. Taylor, Mr. Hansel, and Mr. Clarke, on the methodology appropriate for the statistical psychologist, both Mr. Gregory and Professor Burt have appealed to the so-called ' uncertainty principle '. They believe that this principle operates within the nervous system itself, and so implies the presence of an irreducible random element in all psychophysical reactions.

Now one very familiar technique which is based on the hypothesis of random reactions is Thomson's ' sampling theory ', described in his *Factorial Analysis of Human Ability*, and interpreted in concrete neurological terms. Guilford in his *Psychometric Methods* (pp. 465 *et seq.*) enumerates its many merits, and commends it as a basis both for theory and practice. Still more recently Herbert Solomon has approved it as " framing and formulating a new theory of mental ability " (*Mathematical Thinking in the Measurement of Behaviour*, 1960, pp. 285 *et seq.*). It is therefore surprising that, although the *British Journal of Statistical Psychology* has discussed almost every other method of factor analysis, it scarcely ever mentions this model. Since it claims to publish, not only current research, but also historical studies indicating the problems and procedures from which current researches have arisen and expository notes and papers for the ordinary student explaining the techniques employed, surely the sampling theory deserves a brief description.

Journals of statistical psychology are too apt to restrict themselves to what Guilford has called " exercises in statistical manipulation "[1], and to concentrate on abstract mathematical models and methods for their own sake with little or no reference to the concrete neurological processes which must after all be thought of as determining the statistical variables or factors involved if those factors and variables are to have any psychological meaning. For the contemporary behaviourist the special advantage of the sampling theory is that it offers an explicit physiological framework for statistical techniques and for the construction of psychological tests and psychiatric hypotheses. It links the results of the factorist with the actual activities that presumably go on within the brain. For the contemporary psychiatrist it provides an attractive explanation of mental breakdown far superior to those put forward by other factorists. According to Spearman, if I understand him aright, mental breakdown is the result of excessive tension set up by excessive cerebral energy, resulting in what Burt calls mental ' turbulence ' (though, if we are to have physical analogues or models, I should have thought models drawn from electrical theory more appropriate than hypothetical models or metaphors derived from hydrodynamics). According to Thomson's theory mental breakdown and confusion would be the result of an excessive random element producing chaotic reactions—a far more plausible explanation.

Moreover, factors as such need no longer be reified. In their place we have a sampling of nerve-cells and neural arcs. Tests excite chains of stimulus and response. Abilities are no mere faculties : they are just samples of nerve-cells. And the essential nature of a

[1] See his criticisms of previous articles in *Psychometrika* and other journals of statistical psychology in the current issue of that periodical (XXV, 1961, p.122).

test is " to activate a random sample of the nerve-cells of the brain ". Further, as we are now told, all learning—even the learning of a conditioned reflex—proceeds by trial and error, or, in other words, by sampling—random at first, and progressively restricted by ' reinforcement ' during the phase of practice. This is admitted by Burt and Foley in their " Statistical Analysis of the Learning Process " (this *Journal*, IX, pp. 49 *et seq.*) ; but they make no attempt to interpret their assumptions in neural or cerebral terms. If Professor Burt rejects Thomson's very attractive neurological hypothesis, what alternative would he suggest in its place ?

In his discussions on the ' uncertainty principle ' Professor Burt seems rather to shirk the problem from which it all stemmed—the problem of ' will as such '. I suggest that what seems like the ' certainty of will ' is really an effect of learning, operating to reduce the uncertainties of trial and error. The brain thus becomes the ' organ of will '.

May I add that, so far as I can see, Thomson himself does not assume any *special* type of ' uncertainty ' operating in the brain ? For him the sampling of ' bonds ' or nerve-cells in no way differs from the sampling of cards or dice. As a behaviourist I should hold that *all* the processes of the brain—including even the slips and accidents that every examinee is apt to make—are rigorously determined, just like the fall of a coin or a die. If the physiological psychologist maintains the contrary, he should explain in detail how he conceives the processes that he postulates. And perhaps in his reply he will remember that most statistical psychologists, and even, I would say, most psychiatrists, really know very little about the working of the brain other than what can be found in the elementary textbooks of physiology which they thumbed over in their student days.

The brain, the nerve-currents in the brain, and the material stimuli that excite the brain, are all of them physical entities in a three-dimensional physical space. The sensory percepts of the person who owns the brain also form a spatio-temporal continuum. Is it not therefore possible to regard these various patterned spaces as part of one all-embracing space (or rather 'space-time') ? And in that case could not this hyperspace be regarded as a kind of 'factorial space' like that depicted in the three-dimensional diagrams printed by Thomson, Burt, and Cattell—e.g., *Brit. J. Statist. Psychol.*, VIII, p. 110 ? Here the first dimension represents *time* ; the second (a non-personal dimension allocated to tests or stimuli) really represents a projection (on a single line) of *physical space* and the events within it ; and the third, labelled persons, represents in linear projection the various *mental spaces* of individuals concerned. But since both physical space and each mental space are three-dimensional, we have a psychophysical factor-space which, like that described by Dr. Smythies, has in all $3(m+1)$ dimensions, m being the number of individuals (cf. his *Analysis of Perception*, 1956, pp. 29, 129f.). The study of this psychophysical factor-space is surely a task for the statistical psychologist, and, if successful, might lay the foundations for a psychophysical cosmology that would take the place of traditional metaphysics.

This mode of interpreting factorial results comes nearer to that adopted by Thomson than that favoured by Spearman or Thurstone or Burt, and again illustrates the fruitfulness of the sampling theory. The theory easily covers the problem of individual differences, since according to Thomson tests may be regarded as ' sampling men ' as well as ' sampling abilities ', though he himself does not work out this suggestion. However he accepts Burt's conception of ' person factors ' in a 'person space' and 'test factors ' in a 'test space', as well as his contention that there are certain ' reciprocal relations ' uniting the two. It would seem therefore to follow, on Thomson's own principles, that both kinds of space—the individual's space and the space of physical stimuli or tests—become partial subspaces of a common hyperspace.

All this implies three overruling neurological conditions which the statistical psychologist should keep in mind in constructing the abstract theories which he proposes to test, viz., the way the brain functions in perception, in learning, and in what is popularly called willing. To each, if Thomson is right, the clue is random sampling.

Lewis R. Levy.

FACTOR ANALYSIS AND ITS NEUROLOGICAL BASIS

A Reply

By Cyril Burt

Dr. Levy has raised a variety of questions all of which undoubtedly deserve fuller consideration from statistical psychologists than they have hitherto received. Nevertheless, many are primarily problems in general psychology or neurology, and can therefore only be touched on very briefly here. In the main I shall confine myself to those questions which have a fairly direct bearing on factorial techniques and on the interpretation of psychological ' factors ', and leave the remainder to another occasion.

I. The Sampling Theory

The Sampling of Neural Bonds. Thomson's sampling theory was originally put forward as an alternative to Spearman's one-factor theory. Spearman's model sought to explain the correlations between mental tests without invoking group factors; Thomson's model sought to explain them without invoking any general factor. His own belief, so he tells us, was that the " near-hierarchical arrangement ", on which Spearman laid so much stress, was " produced by a number of group-factors which are of wide range and not mutually exclusive " (i.e., which overlap).[1] There was a third and still older theory—the theory proposed by Galton and adopted by McDougall, Flugel, and myself—involving the two-fold assumption of a single general ability supplemented by a number of special aptitudes, " a combination of old-fashioned faculties with a new-fangled super-faculty ", as Thomson described it. This, so he contended, was in conflict with the current conception of the mind or brain inculcated by the associationist and behaviourist schools which were then once more in the ascendant.

Spearman himself identified the general factor with ' cerebral energy '. I identified it with the general structure of the brain. Thomson, challenged to interpret his theory in neurological terms, suggested that his " factors or elements " could be " identified on the mental side with Thorndike's associations or ' bonds ', and on the bodily side with neurons or neuron-arcs ". And he argued that, " so far from showing that the mind or brain has a definite structure " (as McDougall and his followers held[2]), " the low rank of the near-hierarchical correlation tables really means that it has hardly any structure at all ".[3] Thomson in short adopts what Sholl has called the ' bowl-of-porridge ' conception of the brain.

The nerve-cells or neuron-arcs are described as " firing on an all-or-none basis ". Thus, unlike the older type of group-factor, Thomson's ' elements ' or ' factors ' have no quantitative measurements attached: each counts for 1 if it influences performance at a test and for 0 if it does not. The brain is supposed to possess a large but finite number of these ' elements '; and every test takes a random sample of them. Nevertheless, different tests differ greatly in ' strength or richness ': some take large samples, others take small. And in their sampling operations they are very apt to overlap, the ' richest ' naturally tending to overlap the most.

Hierarchy without a General Factor. The details of the theory will become clearer if we examine Thomson's discussion of his own example in the light of his earlier papers.

[1] W. Brown and G. H. Thomson, *The Essentials of Mental Measurement*, 1925, pp. 174f.

[2] W. McDougall, *Psychology*, 1912, chap. III, ' The Structure of the Mind ': cf. C. Burt, ' The Structure of the Mind: A Review of the Results of Factor Analysis ', *Brit. J. Educ. Psychol.*, XIX, 1949, pp. 176f., and refs.

[3] G. H. Thomson, *The Factorial Analysis of Human Ability*, 3rd ed., 1948, pp. 303f.

This, I think, will give a view of his theory somewhat different from that which Dr. Levy has apparently derived from brief accounts in American textbooks. For simplicity, Thomson supposes that the mind " can form in all 108 bonds or some multiple of 108 (the number is chosen merely because it leads to no fractions) ". Four tests are applied. They sample 5/6, 4/6, 3/6, and 2/6 respectively of the total number of bonds; i.e., the samples will contain 90, 72, 54, and 36 bonds respectively. Now " these fractions ", we are told, "are the same in value as the communalities"; and on working out the correlations we obtain the coefficients shown below. To elucidate the way Thomson's table (*op. cit.*, p. 43) has been constructed I have inserted both the communalities and the factor saturations: the latter can be obtained by using either ' weighted summation ' (with the vulgar fractions) or ' simple summation ' (with the equivalent decimal fractions).[1]

TABLE I. THOMSON'S TABLE OF CORRELATIONS

Communalities and Saturations for the General Factor added by Burt

Test / Factor Saturation	1 $\sqrt{(5/6)}$	2 $\sqrt{(4/6)}$	3 $\sqrt{(3/6)}$	4 $\sqrt{(2/6)}$
1 $\sqrt{(5/6)}$	[5/6]	$\sqrt{20/6}$	$\sqrt{15/6}$	$\sqrt{10/6}$
2 $\sqrt{(4/6)}$	$\sqrt{20/6}$	[4/6]	$\sqrt{12/6}$	$\sqrt{8/6}$
3 $\sqrt{(3/6)}$	$\sqrt{15/6}$	$\sqrt{12/6}$	[3/6]	$\sqrt{6/6}$
4 $\sqrt{(2/6)}$	$\sqrt{10/6}$	$\sqrt{8/6}$	$\sqrt{6/6}$	[2/6]

" The tetrad-differences ", as Thomson points out, " are all zero by inspection "; hence the table *could* be explained by a single general factor. Thomson himself however puts forward an " alternative explanation " which assumes that the size of each correlation is determined by the number of bonds common to the two tests. The amount of overlap he demonstrates by constructing a diagram of overlapping ovals, rather like those popularized by Euler to illustrate deductive arguments: the overlapping parts he calls ' cells ' (with no allusion of course to nerve-cells). With the experimental data before us we can calculate the ' richness ' of each test (i.e., their communalities); but we cannot " tell *with certainty* what the contents of each cell will be; we can only calculate the *most probable* contents ".

We proceed as follows. " The number of bonds used in all four tests is most probably

$$\tfrac{5}{6} \times \tfrac{4}{6} \times \tfrac{3}{6} \times \tfrac{2}{6} \times 108 = 10 \text{ bonds} ".$$

Similarly the most probable number used in the first three tests (tests 1, 2, and 3) will be

$$\tfrac{5}{6} \times \tfrac{4}{6} \times \tfrac{3}{6} \times 108 = 30 \text{ bonds.}$$

But 10 of these have already been included in the set common to all four: that leaves only 20 bonds entering into these three and no more. Similarly there will be 10 bonds entering into tests 1, 2, and 4 and no more. Finally, the number entering into 1 and 2 will be

$$\tfrac{5}{6} \times \tfrac{4}{6} \times 108 = 60 \text{ bonds.}$$

But 40 of these have already entered into tests 1, 2 and either 3 or 4 or both. That leaves only 20 peculiar to just these two.

The final result will be clearer if we enter the figures in the columns of a table rather than in a diagram of ovals. They are set out in Table II. The arabic figures (1, 2, 3 or 4)

[1] This method of accounting for correlations appears to have been suggested by experiments carried out by Weldon in Pearson's laboratory. His object was to obtain correlations by throwing known samples of dice or coins and treating a certain number as common to both correlated variables: see W. F. R. Weldon, ' Inheritance in Animals and Plants ', ap. *Lectures on the Method of Science* (T. B. Strong, ed.), 1906, pp. 100f. Thomson's earliest attempts at producing a ' hierarchy with no general factor ' were carried out by throwing dice to represent ' common ' and ' specific ' factors exactly as Weldon had done, and using Weldon's formula.

in the top half of the table indicate which tests enter into the group factor specified, and the figures in the bottom half the number of ' bonds ': thus 10 bonds are common to all four tests, 20 to the first three only, and so on. The figures in the last column but one show the number of ' bonds ' specific to each factor; except for reasons of space each should be printed with a column to itself.

TABLE II. GROUP FACTOR MODEL: NUMBER OF OVERLAPPING BONDS

	Group Factors											Specific Factors	Total
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	
	1	1	1	1	1	1	1	–	–	–	–	to	
	2	2	2	–	2	–	–	2	2	2	–	XV	
	3	3	–	3	–	3	–	3	3	–	3		
	4	–	4	4	–	–	4	4	–	4	4		
Test													
1	10	20	10	5	20	10	5	–	–	–	–	10	90
2	10	20	10	–	20	–	–	2	4	2	–	4	72
3	10	20	–	5	–	10	–	2	4	–	1	2	54
4	10	–	10	5	–	–	5	2	–	2	1	1	36

It will be seen that Thomson's method of analysis postulates as many factors as there are combinations of tests taken n, $(n-1)$, $(n-2)$, ... or 1 at a time: i.e., 2^n-1 factors in all; of these n will be specific: some of course might have *no* bonds. Here, for example, he assumes the operation of 11 overlapping group factors and 4 specific factors—15 factors in all. The numbers in the lower half of the table can be regarded as indicating the variances contributed by the several factors: hence $\sqrt{(10/90)}$, $\sqrt{(20/90)}$, ..., $\sqrt{(10/72)}$, $\sqrt{(20/72)}$, ..., etc., would represent their factor loadings. Thomson, however, prefers to express each correlation, not as a product of the factor loadings, but rather in terms of the total number of common bonds; and for that purpose he borrows Weldon's formula

$$r_{ij} = \frac{\text{Number of elements common to both tests}}{\text{Geometric mean of total number contained in each test}}$$

$$= \frac{c}{\sqrt{(t_i \times t_j)}} = \frac{c}{\sqrt{\{(c+s_i)(c+s_j)\}}}. \qquad (2)$$

Thus from the table above (or from the corresponding diagram in Thomson's chapter) we can count up that there are $10+20+10+20=60$ elements or bonds common to tests 1 and 2. Hence

$$r_{12} = \frac{60}{\sqrt{(90 \times 72)}} = \frac{\sqrt{20}}{6}.$$

But evidently, since we really need to know only the *total* number of bonds common to the two tests (60 in the case of tests 1 and 2), there is no need to split it into a series of additive portions as shown in the table or Thomson's diagram.

Alternative Interpretations. Let us now express Thomson's procedure in the more familiar notation. If r_{ig} denotes what would ordinarily be called the general factor loading for the ith test (first column in Table I), then r^2_{ig} will denote the factor variance (or ' communality ', as Thomson prefers to call it); and if T designates the total number of bonds available (here 108), then " the number of bonds required by the test " will be $r^2_{ig}T$; and ' the most probable number of bonds common to two tests ' will be $r^2_{ig}\, r^2_{jg}\, T$.

Thus, by eqn. (2),

$$r_{ij} = \frac{r^2_{ig}\, r^2_{jg}\, T}{\sqrt{\{(r^2_{ig}\, T)(r^2_{jg}\, T)\}}} = r_{ig}\, r_{jg}. \qquad (3)$$

But this is the ordinary product-formula for calculating a correlation on the assumption that only a single general factor is involved. Hence Thomson's procedure is here algebraically identical with that commonly adopted for factorizing a correlation-table of rank one into a series of saturations (or loadings) for one common factor only.

Implications of the Theory. Thomson's sampling theory, when it first appeared, did much to bring home to psychologists the real nature of correlation. Up to then the popular notion had been derived from the early Galton–Spearman analysis of a single correlation coefficient with only one common factor and an independent specific factor, one peculiar to each test. Thomson's approach emphasized the need to consider the whole matrix of intercorrelations, and to regard each and all the correlated variables as being, quite possibly, functions of a number of ' causal elements ' or ' factors '.[1] The notion that " mental activity depended for its efficiency on the working of a large number of brain-cells " provided a convenient illustrative basis for a concrete exposition of the mathematical derivation—a derivation which few psychologists in those days could follow when stated solely in the abstract notation of the integral calculus and determinantal equations, after the fashion regularly adopted by Karl Pearson. But I find it difficult to believe that as " a neurological interpretation of factor theory " the hypothesis can claim any literal validity today.

The chief difference between Thomson's approach and my own may, I think, be stated as follows. Thomson was interested primarily in the description of the *abilities* tested and in the differences between those abilities; I was interested rather in the *persons* tested and in the differences between them. In Thomson's theory individual differences play no part, though, of course, the theory does not exclude them. He himself, however, always speaks of ' *the* mind ', as if the successive applications of the tests sampled the same mind on each occasion, not different minds.[2]

[1] This was essentially Pearson's approach as contrasted with Galton's: see Brown's chapter on the General Theory of Multiple Correlation (" based on a lecture demonstration given by Professor Pearson ") in the first edition of *The Essentials of Mental Measurement* (pp. 128–133: The chapter was unfortunately omitted from later editions as too abstruse and sufficiently covered by the chapters on sampling theory).

I fully agree with Guilford's detailed statement of the superior merits possessed by Thomson's ' sampling theories ' as compared with Spearman's ' two factor theory '. On a later page Guilford himself seems ultimately to decide in favour of a ' weighted group-factor theory ', and evidently endorses most of the current criticisms of the sampling theory (*op. cit.*, pp. 467f.). It is too often forgotten that Thomson's theory, like Spearman's, was originally put forward to explain why so many tables of test-correlations had a rank of one and no more. My own answer was: " not because of the way the tests sample the ' bonds ', but because of the way the psychologists have sampled the tests ". In the early small scale studies, the investigator nearly always avoided duplicating his tests, and selected *one* specimen only of each type of cognitive activity. As soon as psychologists started to use large batteries and groups of *similar* tests, then group-factors as well as the single general factor began to appear (see Burt, *Brit. J. Psychol.*, XXX, 1939, pp. 85f.).

[2] This criticism was also urged by Ballard, who, as a school inspector, was equally interested in differences between the pupils tested. Thomson replied by saying that there were no reasons why the theory *must* imply that individual minds were all the same: like the tests, he says, men might also " be samples "; and, like the tests, " some might be rich, some poor in the bonds ". But this assumption nowhere enters into the structure of his theory, nor are we told how the ' richness ' of each man should be assessed. (Several alternative devices are conceivable, analogous to those used in ordinary factor theory; but we do not know which of them, if any, Thomson would have used.)

If we include the possibility that individuals differ in the total number of bonds they possess (or, as I would rather say, the number they can form), then Thomson's account of the general factor does not vary greatly from my own. In my view the predominance of the general factor simply implies that a given person's brain or nervous system is more or less of the same quality throughout: and this is very much what Thomson endeavoured to convey by declaring that the mind or brain was ' structureless '.[1]

II. The Neurological Basis

Neural Structure and Factorial Structure. Dr. Levy asks, " if Burt rejects Thomson's very attractive neurological hypothesis, what alternative would he suggest in its place? " My own view was briefly outlined in one or two earlier papers, and was based on the conception of the central nervous system and its working elaborated by Sherrington. Later work enables us to fill in fuller details.[2]

The central nervous system may be pictured as an immense tridimensional network, consisting of elongated cells (or ' neurons ') and their branches, joined end to end by cell-junctions (or, to use Sherrington's term, ' synapses '). The neuron is a cell specifically adapted for propagating electro-chemical impulses. Hence the whole system forms a highly complicated set of interlacing paths or channels. It is not entirely ' undifferentiated ', and by no means completely ' structureless '. Within the overgrown nerve-ganglion that we call the brain some of the nerve-cells are collected together in certain central cell masses, such as the optic thalamus and the corpus striatum, but most of them are located in the crumpled outer sheet of grey matter, known as the ' cortex ', where they can make easy contact with each other. Consciousness seems to be associated solely with what goes on in these particular regions.

The areas of the cortex may be classified into two main groups—areas directly connected by nerve-fibres to remoter parts of the nervous system and so ultimately with the sense-organs and muscles (the so-called 'projection areas'), and areas *without* such connections (the so-called ' association areas '). The whole structure is arranged in a hierarchy of ' levels ', and reveals slight but unmistakable differences in the different areas. The levels appear to correspond with the group factors for mental *processes* (sensori-motor, associative, relational, and the like); and the areas to group factors for different types of mental *content* (visual, auditory, kinaesthetic, verbal, etc.). But both in the higher mammals and in man the general differences between different individual brains are far more striking than the more specific differences between different parts belonging to the same brain. Thus, as compared with that of the normal person, the cortical tissue of the low grade defective displays comparatively few cells, and these have comparatively few branches; and both cells and branches appear to be irregularly arranged. These distinctive peculiarities appear in all areas and in all levels of the same individual brain. Such a brain is therefore lacking what may be called systematic complexity. It is this general character of the individual's brain-tissue—viz., the general degree of systematic complexity in the neuronal

[1] I recognized minor local variations in different regions. Thomson in the later versions of his theory virtually admitted this contention by suggesting the possibility of ' sub-pools ', though he is inclined to believe that these are mainly due to " environment and education and life ".

[2] Cf. C. Burt, ' Experimental Tests of General Intelligence ', *Brit. J. Psychol.*, III, 1909, pp. 94–177: this paper was written while I was working as Sherrington's assistant at Liverpool, and it owed much to discussions of the problem with him. For a more detailed comparison of the results of factor analysis with theories about the organization of the brain, see Burt, ' The Structure of the Mind: A Review of the Results of Factor Analysis ', *Brit. J. Educ. Psychol.*, XIX, pp. 100–11, 176–99, and refs.

architecture—that seems to me to represent the general factor, and account for the high positive correlations obtained between various cognitive tests.[1]

The Reaction to Mental Tests. How then does the brain react to the various tests which the psychologist applies? In answering this question it will be sufficient to think of the human brain, like the rest of the body, as a machine operating according to the ordinary laws of physics and chemistry—but certainly, if it is a machine, it is by far the most complicated machine in existence, working on an exquisitely small scale. As with other machines we may regard its efficiency as depending upon (*a*) its structure and (*b*) the energy which runs it. Roughly, but not very precisely, these two aspects correspond to the ' cognitive ' and the ' conative ' aspects of human activity.

The cortex is a convoluted layer of tissue having an area of about 2,400 cm^2 (roughly 400 sq in.), but a thickness of only about 10·24 cm (1/10 in.). This implies a total volume of about 570 cm^3. But each square millimetre contains about 50,000 nerve-cells; and an average brain contains nearly 93,000,000,000 nerve cells.[2] Now this of itself makes Thomson's sampling theory extremely difficult to accept; for his theory requires that, when the correlation between two tests is high, each test must sample almost the whole of the nerve-cells in the brain (in Thomson's example between two-thirds and five-sixths); and it is impossible to believe that one test alone could sample as many as 70,000,000,000 cells.

So far as we know the nerve-cells in the brain that seem specially concerned with conscious processes do not differ fundamentally in their qualitative properties from nerve-cells elsewhere—those, for example, in the grey matter of the spinal cord which have no direct relation to consciousness. But the information about the outer world as it emerges in consciousness exhibits striking qualitative differences: sounds are different from smells or colours, and colours may be red, green, or blue, and so forth. But neurons going to, and through, and from the brain conduct only one kind of change—an electro-chemical wave travelling at about 300 feet per second. Thus information as received and transmitted by the brain displays no qualitative variations: it is, so to speak, coded in the guise of a set of similar signals differing solely in their number, their locality, and their mutual relations. We should therefore be able to handle it by purely quantitative methods. However, as is obvious from the vast numbers of items or events concerned, we are confronted with " a system comprising components whose interactions can only be described in terms of probabilities ". Hence, in spite of the objections advanced by Dr. Taylor (this *Journal*, XI, pp. 133f.), a *statistical treatment* is both appropriate and essential (Sholl, *op. cit.*, pp. 62f.).

[1] This hypothesis was put forward fifty years ago in a paper describing correlations between tests for various intellectual processes. " If we ask how we are to picture the basis for a mental ' factor ' or trait so general and yet biologically transmitted, I suggest we can identify it with the strength of a developmental tendency towards systematic structure in the central nervous system or brain. It is this that furnishes the possibility of an ever-changing adaptation to an ever-changing environment, and adaptation is the prime function of mind, that is, of attentive consciousness. A mind therefore will be most efficient where the inborn capacity for *complex organization* in the nerve-tissue . . . is richest " (Burt, ' The Experimental Study of General Intelligence ', *Child Study*, IV, 1911, pp. 21-23).

[2] H. B. Thompson, ' The Total Number of Functional Nerve Cells in the Cerebral Cortex of Man ', *J. Comp. Neurol.*, IX, 1899, pp. 113–140. This is the estimate still most frequently cited. Later investigators on the whole give rather lower figures (between 5×10^9 and 7×10^9 neurons): the chimpanzee has only about three-quarters that number, and *Cercopithecus* only about a quarter (G. A. Shariff, ' Cell Counts in the Primate Cerebral Cortex ', *J. Comp. Neurol.*, XCVIII, 1953, pp. 381–400).

A single neuron operates on a power of about one or two thousand millionths of a watt; so that the whole brain operates on a power of between 10 and 20 watts.[1] A single nerve-impulse passing across a cell-junction produces a transient electrical charge equivalent to about 0·001 V lasting for about 0·01 sec or a little more. To cause a neutron to discharge, however, an impulse of nearly 10 times this voltage is required. By means of its branches a single neuron may impinge on a hundred or more neurons either in the neighbourhood or in more distant parts; but only about half-a-dozen will, as a rule, be in a state of critical excitability. In that case the cumulative effect of impulses arriving from a number of neurons at once will be to *increase the probability* that some or all these half-dozen will discharge in their usual all-or-none fashion. If then at each successive relay stage every neuron is in potential connection with a hundred more, it is clear that in a fraction of a second an incoming impulse may spread through the ramifying reticulations of the nerve-net to thousands of other neurons. The passage of a nerve-impulse in a ' neuronic arc ' is therefore more like the passage of a wave front than of a simple current along a wire.

The Objections to a Theory of Random Sampling. No doubt in certain cases the course of the excitation may be to some extent a matter of chance. When you or I are meditating on a problem that calls for an imaginative solution, introspection will often suggest that ' chance ' (or, as some psychologists prefer to say, ' the unconscious ') is in the end responsible for a satisfactory answer; and work on trial-and-error processes fully confirms this inference. To some extent, therefore, the brain is undoubtedly a probability machine. I vividly recollect, while I watched Sherrington stimulating the cortex of some anaesthetized animal, how uncertain were the effects of stimulating precisely the same spot. As Sherrington puts it, " every increase in the number of links composing a nerve-cell chain seems greatly to increase the uncertainty of its reactions ". Nevertheless, the uncertainty is never complete. Though the variations are at times bewildering, regularity seems on the whole the general rule.

It appears therefore impossible to accept the view that the *essential* nature of a test is to " activate a random sample of the nerve-cells of the brain ". During states of sleep or inattention the diffuse and random activity of the cells, going round and round in reverberating circuits more or less in phase, would presumably set up the well-known ten-to-a-second alpha rhythm. An incoming sensory stimulus, however, would at once substitute a definite pattern of activity; and, according to the sense-organ from which they come, the impulses, still preserving their distinctive arrangement, will as a rule find their way to the appropriate regions of the cortex, and ultimately, if unimpeded, to what are the more or less appropriate motor centres. And when the outcome is successful, there seems comparatively little that could be put down to the sheer haphazard processes of random sampling.

Certainly, according to patients' reports, an electrical stimulus applied *direct* to the cortex does at times produce something rather like a random sampling of conscious effects—noises, coloured lights, odd skin sensations, differing unpredictably from time to time. Bang your ulnar nerve against the ' funny bone ' in your elbow, and you get a random sample of all the possible sensations which the nerve could convey. But with the normal exploration of surfaces by the hand, the same nerve conveys a pattern of impulses, and you become aware of a significant tactile pattern corresponding closely to the pattern of the stimulating surface. And in much the same way, as I have argued elsewhere, an effective test of intelligence " operates by *selecting* neural bonds rather than by *sampling* them ". Nor does it merely select; it organizes the bonds and the activities they mediate into systematic schemes of a complexity varying with the organizing skill of which the individua

[1] 1 watt $=10^7$ ergs per sec or 33,000 ft. Lb. per min, i.e., 1/746 h.p. In terms of heat, 1 watt $=0{\cdot}057$ British Thermal Unit per min. If the power that runs the brain were used to boil water instead, it would need 3 hours to boil a pint; used for lighting it would operate only a very dim electric bulb to illuminate a passage.

is capable. In short, to borrow Sherrington's phrase, the essential function of the mind or nervous system consists, not in its sampling action, but in its ' integrative action '.[1]

Now for quantitative purposes we might as a first approximation treat complexity as a function of the number of possible connections. Accordingly, if there were no significant specialization in structure or function in the different regions of the brain, we might conceive the general factor of intelligence as measurable by the *average* number of possible connections per unit volume of the individual's brain. Since areas and layers do differ, I suggest it would be better to express it by the *minimal* number of possible connections per unit volume averaged over all the brain. The exceptional efficiency of particular processes might then be expressed by the *additional* number of possible connections in the different areas, i.e., the areas for vision, hearing, speech, etc.: these would appear as additive group factors.[2] Finally, the uncertainties involved in making the right connections could be most conveniently treated as forming an independent factor of ' unreliability ', part of what the statistical psychologist calls the ' specific factor ' (or ' uniqueness ') of the test. ' Slips and accidents ' of this type (often resulting from quite extraneous circumstances) are no more to be explained in terms of the ' sampling theory ' or the ' principle of uncertainty ' than the fact that Ben Walters' bullet " went wildly astray and hit the wrong chap ". From the physicists' standpoint such ' slips and accidents ' are quite as much the result of strict causal determination as the trajectory of the bullet. The uncertainty involved in Heisenberg's principle is of a very different type: it marks an issue which, so far as physical influences are concerned, is *entirely* undertermined.

The Uncertainty Principle. My allusions to the so-called uncertainty principle arose out of questions relating, not to ' intelligence ', but to ' will '. Mr. Clarke, quoting Hebb and F. V. Smith, had argued at some length that " as a science, modern psychology assumes that all behaviour is strictly determined ", and that " physical science, being based on strict causation, leaves no room for what is commonly termed ' free will ' "; other writers, like Taylor and Hansel, have adopted much the same point of view. One or two, indeed, like Dr. Levy, do not wish to deny the uncertainty principle as a principle of micro-physics; but they maintain, as he does, that the neural processes of the brain are on too large a scale to be affected by the operation of that principle, even if we give it our assent.

The problem of size can be settled quite simply. What are popularly called ' nerve-cells ' (the cell bodies of neurons) vary greatly in their bulk. The giant pyramidal cells measure as much as 60 to 80 microns in their diameter; but some of the smaller types measure only 4 to 6 microns. But here we are concerned, not with nerve-*cells*, but with nerve-*junctions*. The fibre-like branches of the nerve-cells subdivide into twig-like filaments; and in the spinal cord these filaments are seen to end in little buttons or knobs (*boutons terminaux*) which are applied to the cell bodies of the adjacent neurons or to their branches. There is no structural continuity between one neuron and another: they are apparently separated by a film that may perhaps consist of just one molecular layer. On the surface-phenomena which thus arise, all the properties of the cell-junction or ' synapse ' essentially depend. In particular this tiny gap and the chemical substances

[1] *Brit. J. Psychol.*, XXX, pp. 87f.

[2] This highly simplified scheme will fit a simple verbal test of intelligence, such as Terman's vocabulary test, quite neatly. But such tests ignore the process of efficiently *combining* connections into an organized whole that is so distinctive a feature of the higher types of intelligent process. Nevertheless, that is not so serious as it might seem. In general the brain can only work with an increased number of connections when it is able to organize them into a system: try tapping with the hand and feet while nodding the head; it is almost impossible to effect the connections required in a random order; inevitably they fall into a rhythm. However, to make sure of testing the capacity for organization I used relational tests in which multiple connections were involved, e.g., tests of implicit or explicit reasoning, and then counted the number of correct combinations made in carrying it out. Such a test of course departs still further from the process of ' taking random samples of nerve-cells ': (cf. *J. Exp. Ped.*, I, pp. 93f., *Brit. J. Educ. Psychol.*, XIX, pp. 176f.)

that occupy it, interpose a varying resistance to the passage of nerve impulses; and, like a valve, permit it to travel in one direction only.

In the cortex the ' synaptic knobs ' either fail to develop or fail to stain. Nevertheless, as measured in the spinal cord they give some indication of the scale of size with which we are here concerned. The diameter of a knob averages about 1 micron; and its mass must therefore be of the order of about 5×10^{-13} gm.[1] Let us now apply Heisenberg's equation.[2]

$$\Delta x \,.\, \Delta p \geqslant \tfrac{1}{2}(h/2\pi),$$

where x denotes position, p momentum, and h is Planck's quantum of action ($6{\cdot}55 \times 10^{-27}$ erg/sec). A little calculation shows that, with a small body having the weight of a synaptic knob, there would be an uncertainty in the actual position of about 20 Ångström units in 1 sec, or about 2 Ångström units in 0·01 sec. Now the gap between the terminals of one neuron and the surface of the next must be only 200 Ångström units across. Evidently an uncertainty of the order indicated might easily introduce an uncertainty into the strength needed by the incoming nerve impulse in order to leap across the gap.[3]

The phrase ' neuron-arc ', used by Thomson and other writers, conveys the notion that the cortical associations or ' bonds ' are effected by end-to-end chains of *single* nerve-cells, each forming a separate track or pathway. This notion perhaps springs partly from the simplified accounts of the ' sensori-motor arc ' that we find in such books as McDougall's *Physiological Psychology*, or Watson's *Psychology from the Standpoint of a Behaviourist* (cf. Figures 25 and 46), and partly from the ever-popular comparison of the nerves to telegraph wires and the brain to a central exchange. But if that were how the brain was constructed, even 90 thousand million cells would not suffice to mediate the highly intricate and highly variable patterns of human behaviour: a simple calculation will show that the mere act of reading this paper would call for more cells than that.

However, what we now know of the organization and functioning of the cortex points to a very different mode of working. As we have just seen, the structure of the cortex reveals, not a system of distinct and specific channels, running from the input-areas to the output-areas, but an intricate three-dimensional, or rather multi-dimensional network; and the most significant feature of every nerve-cell is the way in which its axon (the outgoing fibre conducting impulses away from the cell-body) sooner or later subdivides into numerous branches, and thus makes contact not with one neuron, but with a number. To illustrate the possibilities which all this ramification introduces consider the following alternative arrangements. Let A1, A2, . . . , B1, B2, . . . , C1, C2, represent cell-bodies, and suppose for simplicity that the axon from each subdivides into four branches only, and that impulses from at least two branches are needed to make the next neuron discharge. Impulses arrive

[1] R. A. Haggar and M. L. Barr, ' Quantitative Data on the Size of the Synaptic End-Bulbs in the Cat's Spinal Cord ', *J. Comp. Neurol.*, XCIII, pp. 17–35.

[2] Cf. ' Quantum Theory and the Principle of Indeterminacy ', this *Journal* XI, 1958, pp. 78f.

[3] A micron is a thousandth of a millimetre. An Ångström unit is one ten millionth of a millimetre. The electrical effect produced by a nerve impulse arriving across a synapse is extremely small and transitory—equivalent to about 0·001 V and lasting between 0·01 and 0·02 sec. An excitation having at least ten times this voltage is needed to trigger off a discharge from the post-synaptic neuron: after firing, several milliseconds are required before it is in a position to fire again.

I should perhaps add that it is now generally believed that the transmission of a nerve-impulse across the synapse is effected by the rapid liberation of a chemical substance as a result of the arrival of the pre-synaptic impulse. The substance (possibly acetylcholine) is presumably stored in the nerve-terminals; its diffusion and momentary combination with the substances of the intervening film break down the resistance, generate a potential, and thus set up a wave of depolarization in the post-synaptic neuron: while it diffuses, it is rapidly destroyed by an enzyme (e.g., cholinesterase) : (cf. W. Feldberg, Gegenwärtige Probleme auf dem Gebiet der chemischen Übertragung von Nervenwirkungen, *Arch. Exp. Path. u. Pharm.*, CCXII, 1950, pp. 64–68).

at A3, A4, and A5, which thus are ready to fire. Now let us suppose that, owing either to past activity or to impulses arriving from some other cells, the synapses at B2, B3, and B4 have their thresholds temporarily lowered: then the incoming stream of impulses will be passed on in the direction of C1, C2, and C3 (Figure 1). If, however, the lowest thresholds are at B4, B5, and B6, then the stream will flow in the direction of C5, C6, and C7 (Figure 2).

FIGURE 1

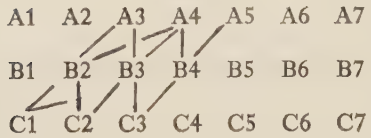
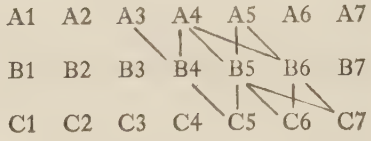


FIGURE 2



This yields a picture of the brain and its working very different from the behaviouristic theory that human conduct is just a matter of stimulus and response, and the brain itself just an organ for conditioned reflexes, " the whole acting like a set of telephone wires with automatic switches". As we now see, there is, or there need be, no specificity about the neural channels. Streams going in very different directions at slightly different times may flow like waves across the same groups of neurons. Further two or more streams may converge and coalesce; or a single stream may conceivably divide into several. Some streams or their offshoots may fade out; others may provide a feedback; and others again may go round and round in circles passing and repassing over the same set of nerve-cells.

It was largely on these grounds that I suggested the use of a ' hypothetical model derived from hydrodynamics ', and of concepts, terms, and analogies borrowed from the study of moving streams and currents. Applied to states of nervous instability, words like ' turbulence ' were used, not as vague metaphors, but as technical terms having much the same meaning as they are given in the theory of fluids generally.[1] Here are a series of problems and a practicable mathematical technique to which both the quantitative psychologist and the clinical psychologist can usefully turn their attention.

A. The Brain and Perception

Perceptual Space, Physical Space, and Factorial Hyperspace. The physiologist studying the nervous system assumes that the things that he examines—the brain, its constituent cells and fibres, the electro-chemical impulses propagated along the nerves—as well as the apparatus he uses to examine them—are all material objects existing independently of his own perceptions and thoughts, in a three-dimensional physical space. Within the brain the nerve-impulses themselves, as we have seen, form a spatio-temporal pattern. The sensory percepts of the owner of that brain—the coloured patches that form his visual field, the tangible shapes he feels with his hands—also form spatio-temporal patterns. And these two patterned schemes—the neural and the sensory—are related in various ways to the pattern of events in the physical space surrounding the owner's brain and body. They

[1] When a fluid, whether liquid or gas, flows with a moderate and even velocity through a tube or other channel having a smooth, uniform and relatively narrow bore, the flow is said to be ' streamline ', i.e. the velocity contains no components transverse to the main direction of the stream. If the bore is irregular, then, as the pressure at the inflow is increased, a critical point is eventually reached at which the type of motion changes; eddies and minor currents are formed, and as a result the flow becomes erratic both in rate and in direction: such a flow is described as ' turbulent '. This conception, as it seemed to me, served very well to describe the condition underlying certain states of intellectual confusion and emotional disturbance. Dr. Gregory has adopted a similar model to account for what, to the clinical observer, seems like a qualitative change in the behaviour of neurotic and psychotic individuals under certain conditions of pressure and stress (cf. this *Journal*, XI, pp. 211f. and refs.).

appear in fact to be initiated by stimulating sources located within that space; and they form his chief means of discovering what is happening there.

Dr. Levy asks whether it is possible to regard these three spatial schemes as parts of one all-embracing space or rather space-time; and he suggests that this all-embracing space-time may be regarded as a kind of " factorial space ", like that displayed in my diagram in an earlier issue of this *Journal* (VIII, p. 110). Of the three dimensions there depicted one explicitly represents ' time ': the second—a non-personal dimension allocated to ' tests '—really represents, he says, ' a projection (on a single line) of physical space and the events within it '; and finally the third, labelled ' persons ', represents (in linear projection) the various ' mental spaces ' of individuals concerned. Now physical space is three-dimensional; and each ' mental space ' is, in his view, also three-dimensional. He thus arrives at a ' psychophysical factor-space '—a hyperspace which, like that described by Dr. Smythies,[1] has $3(m+1)$ dimensions, m being the number of individuals. The study of this psychophysical space, he contends, is essentially a task for the statistical psychologist, and, if successful, might lay the foundations for a " psychophysical cosmology that would take the place of traditional metaphysics ".

Incidentally, he tells us, this mode of interpreting factorial results comes closer to that adopted by Godfrey Thomson than to that favoured by Thurstone, Spearman, and myself; and once again illustrates the fruitfulness of the sampling theory.

The Ambiguity of Space. Dr. Levy does not himself develop any detailed arguments in support of these views, but refers the reader to Dr. Smythies. His extension of Dr. Smythies' conclusions, however, seems to bring out very clearly what I hold to be a manifest fallacy common to both. In my view both of them seem to have taken a highly ambiguous concept ' space ' and have applied it to very disparate sets of phenomena; and by this means they claim to have shown that all these phenomena are part of a single all-inclusive whole, which is also spatial.

In factor analysis the use of geometrical terms and spatial concepts is no more than an expository device. It furnishes a convenient language. Two or more variables that vary together can be represented either by the algebraic symbolism of numbers and letters or by the geometric symbolism of points, lines, planes, and angles. The psychological factorist freely exploits both methods: but, when he makes use of the geometric symbolism and talks of a factor ' space ', he does not for a moment intend the ' space ' to be regarded as a concrete entity.[2] In any case the diagram mentioned, like Thomson's spatial charts, would need a new interpretation before it could be identified with a psychophysical hyperspace: for instance, what Dr. Levy calls the ' non-personal dimension ' describes the common characteristics, not of the physical universe, but of the persons tested (or, in Thomson's illustrations, their abilities).

[1] J. R. Smythies, ' Three Classical Theories of Mind ', *J. Soc. Psychic. Res.*, XL, 1960, pp. 385–96, and *id.*, *Analysis of Perception*, 1956, pp. 29, 129f.

[2] From what he has said elsewhere it is clear that Dr. Levy is well aware of the symbolic nature of the terms used by the factorist; and possibly he would reply that the terminology used by physicists to describe physical space, and the terminology used by philosophers to describe perceptual space are at bottom themselves only symbolic. But in that case the whole argument proves nothing. I might just as well argue that because the symbol p is used by a geographer to denote a point, by a statistician to denote a probability, and by an economist to denote a profit, therefore points, probabilities, and profits are really the same.

The preference for geometrical procedures is well marked with those factorists who, like Thomson and Thurstone, are avowed visualizers; a preference for algebraic procedures is more usual with those whose visual images are, like my own, evanescent and dim. Like Lord Kelvin, the visualizer commonly feels he has mastered a problem when he has hit on a way to picture all the details in a visible model. I cannot help suspecting that Dr. Smythies' descriptions of his own experiences suggest that he at any rate possesses vivid visual imagery; and I wonder whether Dr. Levy may not enjoy the same advantage.

The Representative Theory of Perception. Dr. Smythies' views may best be summarized in his own words. The hypothesis he wishes to support is as follows: " Sense-data and images are spatial entities distinct from physical objects, and bear both temporal and causal relations and higher dimensional spatial relations to physical objects ".[1] He claims two special advantages for his theory over " the traditional dualism ". In opposition to the Cartesian doctrine, which makes mind an unextended and non-spatial substance and so lands us with the anomaly of psychophysical interaction, the new theory holds (i) that " mind and body form a single organism of which they are different *spatial* parts " and (ii) that we are thus enabled to give " an intelligible account of sense-data and the rôle they play in perception ". For if mental events are themselves ' spatial entities ' in the same hyperspace as the external stimuli or the nerve processes in the brain, then, it is maintained, we can validly account for those entities in terms of cause and effect. This in turn will justify us in treating our sense-data as supplying a representation or photographic copy of the events in the physical universe. And on the basis of this interpretation Dr. Smythies concludes that " the mind-brain problem can now be solved by advances in neurophysiology ".

Dr. Smythies works out his " modified version of the ' representative theory ' of perception " chiefly in terms of visual sense-data. But there are other kinds of sense-data—sounds and smells, for example—which possess few, if any, of the spatial characteristics he specifies, and ' mind ' includes many types of ' conscious content ' besides sense-data—pleasure, happiness, anger, imageless thoughts—which have no spatial characteristics whatever. Hence the *whole* mind–brain problem cannot be solved in this way.

Further, the physicists who have accepted the traditional notion of physical causation have almost invariably assumed that the events which act as causes and the events which ensue as their effects are and must be events in the *same* space (or rather the same space-time). Dr. Smythies adduces no evidence whatever for supposing that physical events occurring in ' public ', ' objective ', or ' physical ' space could cause mental events in a ' private ', ' subjective ', and ' non-physical ' space. The mystery of psychophysical interaction therefore seems no nearer solution. And of course the whole notion of causation has of late been vigorously challenged, particularly as applied to the transition from processes in the brain to changes in consciousness.[2]

Dr. Smythies seems to suppose that he is explaining how an observer's private perceptual space could be described in terms of the spatial relations recognized by scientists

[1] This he calls Theory II. Theory I is expressed in similar terms, except that it denies that *spatial* relations connect sense-data and objects. The latter is the theory advanced by H. H. Price (*Perception*, 1950) and more recently by H. Kuhlenbeck (*Brain and Consciousness*, 1957). A final decision between the two hypotheses, says Dr. Smythies, must await ' experimental demonstration '; and, since the entities concerned (including the sense-data) are assumed to be spatial, the demonstration will require the techniques of ' mathematical *physics* ', not, as Fechner and his followers assumed, of mathematical *psychology* or psychophysics. Meanwhile, Dr. Smythies' criticisms of Price's views plainly imply that he himself favours ' Theory II '. It may be added that the idea of a " *space-like* whole of more than three dimensions in which both sensa and scientific objects literally have places " had already been put forward as one possible hypothesis by Broad in his *Scientific Thought* (1927, pp. 392f., 543f.).

[2] E.g., by A. J. Ayer among contemporary philosophers (*The Foundations of Empirical Knowledge*, 1940, pp. 220f.). Dr. Smythies suggests that, with his hypothesis, those who still see difficulties in his theory of psycho-neural interaction could substitute a theory of a psycho-neural parallelism. But here, I suspect, he would find most modern neurologists against him: Sherrington, for instance, declares that " although the physical basis encroaches more and more on the study of the mind, there are mental events which seem to lie beyond any physiology of the brain "; and Adrian observes that there seems to be " an important part of the picture—the part that deals with the mind—which can never be fitted into our picture of the brain " (ap. P. Laslett, *The Physical Basis of Mind*, 1950, pp. 3, 5). The question is largely verbal; and many of the puzzles may be evaded by substituting the language of information theory for that of causal theories.

in their geometrical accounts of physical space; but what in fact he is really doing is to interpret, or rather to picture, the scientist's conception of physical space by analogy with the perceptual space of binocular vision. His aim is to show that the visual field is really a copy or representation of the physical field; but he does this by assuming the physical field is just a replica of the visual. Thus, he repeatedly compares the resemblance between " the perceptions in our minds " (as people say) and " the events outside our minds " with the resemblance between a moving picture as seen on a television screen and the actual events as scanned by the television camera on the spot. He even claims that " the mechanisms of visual perception *are* television mechanisms " (*loc. cit.*, p. 42, his italics); and he warns us against supposing that the comparison is " *only* an analogy ".

There are several difficulties here. On the physiological side, the stream of electrochemical nerve-impulses, as is evident from the description just given, bear no resemblance to a modulated radio-wave; nor is there any mechanism in the eye or brain to perform the scanning that is done by the television camera. On the psychological side, our reflective reconstruction of the physical world is based on a quasi-inferential procedure—namely, on what Spearman would call an ' eduction of correlates '. We have no means whatever of directly comparing the actual physical events with the alleged mental copy in the way we can compare them with the copy on the television screen. And in any case I should have thought the differences were far more startling than the similarities. The physical world is devoid of all secondary qualities (unlike the mind, the ordinary television camera leaves out the colours instead of putting them in). And if, as the modern physicist maintains, the events in the physical world consist of particles gyrating round each other and emitting waves of probability that traverse space-time in geodesics, then none of this finds any representation in our visual field.

However, if we waive these rather dubious analogies, what Dr. Smythies' theory really comes to is this: the individual's ' personal space ' as studied by the psychologist and the ' scientific space ' postulated by the physicist exhibit no differences in kind: they are just " two different sections of an *n*-dimensional space, not two *kinds* of section ". But I doubt if either the psychologist or the physicist would agree with his versions of space as each of them understands it.

(i) To begin with, every individual experiences not one sensible space, but several. Tactile space is different from visual; auditory space (as the introspections of those born blind clearly show) is different from either;[1] when their eyes are open, many people (I for one) locate their visual images in a space which is quite unrelated to any space that is actually perceived. The part played by the proprioceptors is more obscure. Without these, and particularly without the contributions from the inner ear, we should have a very poor notion of direction and orientation: and what meaning can ' up ' and ' down ' have for an astronaut in a windowless capsule when deprived of the sensation of weight? Finally, the temporal integration of kinaesthetic sensation seems essential for the recognition of distances traversed.

Certainly it is quite unsafe to identify the individual's ' private ' or ' subjective ' space with his ' visual space '. This in any case is not a ' space ' (which is an infinite continuum) but a ' field ' (a very restricted type of continuum). In a factorial study carried out by Miss E. A. Hudson the largest contribution to the general factor of subjective space came, not from visual perception, but from the perception of movement; verbal descriptions came second; visual imagery third; visual perception only fourth. Moreover there were wide individual differences. A man's private or ' sensible ' space is therefore, not a simple three-dimensional percept but a highly artificial construction, quite as much conceptual as perceptual. If the man were a physicist, it would not be a *section* of the

[1] It is a little surprising that, since our first ancestors emerged from the sea and became land-animals, we have evolved no new sense-organ for the perception of space or of the energies which operate in space.

same whole as physical space; it would be identical with physical space as he conceives it, for that is also a highly artificial concept.

(ii) As we have seen, Dr. Smythies' own conception of ' scientific space ' seems to resemble a psychological construction based on the space of binocular vision rather than the sophisticated concept of the modern physicist. A questionnaire addressed to students (including students of physics as well as of psychology) indicates that the majority of them picture it, much as Plato did,[1] as a kind of huge box with no sides or bottom, or as a vast room whose ceiling, walls, and floor are infinitely remote, a receptacle in which all physical objects can be contained, but possessing no physical properties or concrete structure of its own. This in fact was the notion of Absolute Space adopted by Newton and most of his contemporaries. But it is not the conception of the modern physicist. For him the word space no longer suggests a great container—an actual three-dimensional framework in which physical substances are located. The term has become simply a convenient name for certain highly abstract relational properties—notably those of order—which characterize the spatio-temporal whole we call the physical universe.[2]

Since subjective or sensible space and objective or physical space are both parts of a single spatial whole, Dr. Smythies argues that " sensa and physical objects must bear spatial relations to each other ". Nevertheless it is hard to see what advantages are gained either by this hypothetical union of the sorts of space or by the somewhat unexpected inference that he draws. I can see a certain plausibility in unifying subjective time with objective time: e.g., in assuming that events in my own conscious life-history form part of the wider series of events that make up the life-history of the universe; something of this sort seems implied when I talk of waking at the proper time to watch the sun rise at the solstice. Yet even here I doubt whether this is the right assumption. However, Dr. Smythies tells us that with his spatial model " the visual field is able to represent a portion of the physical world ", much as a mirror will re-present an object when the two are (so to

[1] *πάσης γενέσεως ὑποδοχή* (' a receptacle for all created things ') Plato, *Timaeus*, 49 A cf 51 A.

[2] This view is as old as Newton's great contemporary Leibniz: " There is no actual empty space or actual empty time. Space is merely relational as time is. I hold it to be an order of possible coexistences " (*Troisième Lettre à Clarke;* cf. *Réplique aux Réflexions de Boyle*, 1702, Erdmann's edition 189b). Leibniz's doctrine, however, was emphatically rejected by Kant: " Der Raum ist kein diskursiver, oder, wie man sagt, allgemeiner Begriff von Verhältnissen der Dinge" (*Kritik der reinen Vernunft*, 1, 1ster Abschn., 2(4)). In Britain, even before the advent of Einstein's relativity, the relational view was supported by Ward among psychologists and by Russell among philosophers (cf. B. Russell, *Critical Exposition of the Philosophy of Leibniz*, 1900, and earlier papers). Today, of course, the relations postulated would be relations, not between *points* in space *or* time, but between *events* in *space-time*. However, until quite recently, most British psychologists, physiologists and philosophers seem to have preferred the Newtonian view. Continental writers have been greatly influenced by the views of Hilbert. For him points may be denoted by *any* undefined elements that obey certain axioms; lines and planes are undefined *classes* of points; dimensionality is not the property of class as such, but of *order* in a class. Moreover, most contemporary physicists, I imagine, would hold that the topological structure of the physical world (i.e. of what Dr. Smythies calls ' scientific space ') is independent of measurable magnitudes. Magnitudes appear with the use of coordinates and the choice of a metric, but they are eventually eliminated as a result of considering only those properties which are invariant when the coordinates and the metric are changed. Thus what the physicist is really concerned with is primarily the *logical* structure of the physical world; and what the layman thinks of as its ' mathematical ' features—the determination of angles, distances, and various other quantities—are introduced merely for the convenience of proof.

Professor Broad brings all this out very neatly by suggesting that, instead of picturing spatial structures in visual terms, we should think of the relations involved as relations between members of a huge family: the various spaces of different persons would then be expressed by the relations subsisting between members of certain sub-families, i.e., of branches of the wider family (cf. *Scientific Thought*, pp. 544f.).

speak) face to face in the same room. Apparently he supposes that sense-data can only yield knowledge of physical objects, if they resemble them.[1] But a mirror-like resemblance is an unnecessary and indeed an improbable requirement. What is needed is merely that the two shall approximately correspond.[2] Without some such correspondence our behaviour would fail to effect its aims; and neither we nor our species could expect to survive. But the correspondence need only be a correspondence of order. The philosopher, and still more often the philosophizing physiologist seem prone to forget that natural selection does not cater for scientific curiosity about the universe: its concern is merely that every life should gain a living.

Correspondence can exist without both corresponding structures being spatial. Even if they were, spatial relations alone could not guarantee what is required. It is the correspondences we must investigate. Since they are never exact enough to amount to strict isomorphism, a correlational technique would seem to furnish the simplest means for most investigations. Here again there are obvious and urgent problems for the statistical psychologist to attack.

B. The Brain and Learning

Mathematical Models. There have been several attempts to construct mathematical models simulating a nervous network with completely random interconnections, supplemented as a rule by devices for reinforcing appropriate connections and eliminating inappropriate connections. Some of the simplest have been embodied in actual working models. The most ambitious perhaps is the ' Perceptron '.[3] From all of them the conclusion to be drawn is much the same: a mechanism based on a completely random network learns extremely little extremely slowly, and needs an extremely large number of elements to accomplish even the simplest type of discriminative reaction. Whenever the task is at all complex, some degree of non-randomness seems essential, though a certain degree of randomness renders behaviour more elastic and so more adaptable; and where learning entails in addition some type of problem-solving, then, it would seem, no computing machine working on an entirely random network can succeed[4].

The Neurological Processes. Far less is known about the neurological basis of learning than Dr. Levy appears to assume. Both Eccles and Kinorski agree that the hypothetical physiological concepts postulated by Pavlov—irradiation, induction, internal and external

[1] The half-unconscious aim seems to be to account for cognition without invoking any sort of specifically cognitive or mental relations. The more familiar causal relations, spatial relations, and relations of similarity are each in their turn treated as capable of doing the job. I hold that it is impossible to dispense with a cognitive relation *sui generis*—a relation of direct awareness, peculiar to those organisms which possess consciousness. But I have set out my arguments more fully elsewhere and need not repeat them here.

[2] I shall not attempt a formal definition of ' correspondence ' here. I regard it as a generalization of what in matrix algebra is called ' equivalence '. An illustration may help. The notes we see on the staves of a printed orchestral score form a spatial arrangement of spatial shapes. They *correspond* with the musical sounds as heard, but in no way *resemble* them. The sounds are not spatial; still less are their relations to the corresponding notes spatial relations. In point of fact, if the contents of our ' private sensory space ' formed a complete facsimile of the physical world it would be most confusing: fortunately our sense-organs select and simplify those crucial features to which it is imperative we should respond if we are to survive.

[3] Cf. N. Rashevsky, *Mathematical Biophysics*, 1948. W. S. McCulloch and W. Pitts, 'A Logical Calculus of the Ideas Immanent in Nervous Activity ', *Bull. Math. Biophys.*, V, 1943, pp. 115f. W. R. Ashby, *Design for a Brain*, 1952. W. Grey Walter, *The Living Brain*, 1953. F. Rosenblatt, ' The Perceptron: A Probabilistic Model for Information Storage and Organization in the Brain ', *Psychol. Rev.*, 1958, pp. 386f.

[4] Cf. R. M. Friedberg, 'A Learning Machine ', *IBM J. Res. and Devel.*, II, 1958, pp. 2f.

inhibition, and the like—" have no neurophysiological or neuroanatomical meaning ".[1] As we have seen, the anatomical arrangement of neurons in the cortex would undoubtedly allow impulses to keep circling around, by a kind of reverberative activity, in closed self-re-exciting chains;[2] and Rashevsky, Young, Householder and Landahl, and others have attributed both learning and retention to these continuously circulating patterns.[3] Such a hypothesis might account for short-distance memory; but it can hardly account for more permanent storage, for it is difficult to suppose that all these circulating patterns persist even during deep or complete anaesthesia.

On the whole the hypothetical model which seems most convincing is that schematically outlined long ago by McDougall: it assumes overlapping of impulses, synaptic facilitation, and a permanent lowering of synaptic resistance.[4] There is now a good deal of experimental evidence to support these several assumptions;[5] and if this hypothesis be accepted, then it means that there must be *some* specificity in the neural pathways. Dr. Levy contends that the statistical studies of learning which Foley and I carried out themselves really imply a hypothesis of random sampling. I would prefer to describe our basic assumption as depending not on a sampling theory, but simply on a stochastic theory; it postulates, not complete uncertainty at the initial stage, but only a partial uncertainty. What, as it seems, adaptation requires is not that the neural responses should be entirely random, but that they should include some measure of freedom. For the rest the process is still one of organization rather than of sampling.

C. The Brain and the Will

The Nature of Will. Dr. Levy complains that, even in the discussions with Mr. Clarke and his deterministic supporters, I have " rather shirked the problem of the will as such ". This was partly because ' will ' is such an elusive concept, and partly because in considering the relations between brain and mind, the study of the cognitive problem is logically prior to the study of the conative and likely to prove far more informative. Many psychologists have denied that anything like ' will ' could affect the brain; but no one can deny that the brain affects cognition.

If we are to discuss the problem of ' will ', we must first agree how the term is to be understood. For the purposes of the present argument I shall take the word to mean

[1] J. Konorski, *Conditioned Reflexes and Neuron Organization*, 1948; cf. *id.*, ' The Mechanisms of Learning ', *Symp. Soc. Exp. Biol.*, IV, 1950, pp. 409f.

[2] Cf. R. Lorente de Nó, ' Studies on the Structure of the Cerebral Cortex ', *J. Psych. Neurol.*, XLV, 1933, pp. 381f. and XLVI, 1934, pp. 113f. B. Delisle Burns, ' Some Properties of the Cat's Isolated Cerebral Cortex ', *J. Physiol.*, CXI, 1950, pp. 50f.

[3] J. Z. Young, *ap.* G. R. de Beer, *Essays Presented to E. S. Goodrich*, 1938, pp. 179f.: A. S. Householder and D. H. Landahl, *Mathematical Biophysics of the Central Nervous System*, 1945. Everyone is aware how a tune or a phrase will " keep going round and round in one's head "; and no doubt iterative processes of this sort, both conscious and unconscious, help to keep alive a ' short distance recollection ', while the ' long distance memory ' is being driven home.

[4] W. McDougall, *Physiological Psychology*, 1905, chap. VIII. Compare his diagram on p. 143 with Eccles' diagrams, *op. cit.*, pp. 220 and 222. The chief difference is that Eccles is thinking in terms of streams or patterned ' columns ' of neuronal activity rather than in terms of single neurons. But that is a modification McDougall would willingly have accepted. It was partly for this reason that I formulated my own arguments in terms of matrix notation and factor analysis: see also the recent work of L. R. Tucker, ' Determination of Generalized Learning Curves by Factor Analysis ', *Psychol. Dept. Univ. Princeton*, 1960; and R. A. Weitzman, ' The Performance of Rats and Fish on a Probabilistic Learning Problem ', *Educ. Testing Serv. Rep., Princeton*, 1959.

[5] There is, for example, direct evidence to show that ' use ' leads to increased functional efficiency in the synaptic junctions and that ' disuse ' leads to diminished function: cf. J. C. Eccles and A. K. McIntyre, ' Plasticity of Mammalian Monosynaptic Reflexes ', *Nature*, CLXVII, pp. 466f., and *op cit. sup.*, pp. 203f.

a conscious, non-physical agency whose existence and activities escape detection by the most delicate instruments that the physicist has been able to devise.[1] With that definition there would seem to be but two ways in which we could become aware of its working. (i) We may have a direct intuitive apprehension of its action. I myself am inclined to believe that this is true. No doubt the alleged experiences of a ' conative effort ', as felt, for instance, when we endeavour to lift a heavy weight, consist in the main, of kinaesthetic sensations. But in wishing, yearning, wanting, and striving to put my wants into effect, I seem to have a direct awareness—even when no overt movement is made—of a genuine conative tendency; and this experience appears to supply the chief source for the anthropomorphic conception of force. However, these rather questionable introspective pronouncements can at most only be cited as confirmatory evidence, not as conclusive. (ii) But secondly, the brain itself may act as a detector. In that case the evidence for our belief in the efficacy of will would be indirect—an inference from the way in which voluntary effort modifies, or seems to modify, our bodily reactions. How far this conception of the brain is permissible or probable is a question to be answered in the first place by the physicist and physiologist; and it is to them the philosopher or psychologist must turn before offering views of his own.

The Physical Possibilities. Among physicists Eddington is the only writer who has examined the issues in some detail. He has at different times advanced three possible hypotheses.

(i) In what I will call his pre-Heisenberg writings he put forward his own version of the idealistic type of monism that had become increasingly common in this country from the days of Mill and Mach. " To state it crudely " (he says) " the stuff of the world is mind-stuff, . . . and within the world the main cleavage is, not between the material and the mental, but between the metrical and the non-metrical ". Physical science deals only with the metrical aspect; mental attributes may be purely qualitative and are therefore non-metrical. Volition he held to be " genuine "; but he considered it to be " one of the most dubious points in the theory of the interaction of mind and matter ".[2]

(ii) A few years later, however, he tells us that " the recent revolution which has expelled determinism from present-day physics . . . is sufficient to justify a reorientation of our attitude towards the problem [of free will] ". And in this he has been followed by a number of younger quantum theorists. Yet " though the door of freedom is opened, it is not flung open wide ". Overt bodily behaviour depends " on a kind of trigger action; a minute physical change in a nerve may release a vast amount of chemical energy pent up in the muscles ". If therefore we accept the uncertainty principle (as most physicists do) it becomes conceivable that, within the very narrow margin which that principle allows, mind might behave as a kind of psychokinetic mechanism able to produce these " minute physical changes within a nerve ".[3]

(iii) Nevertheless, after some hesitation, Eddington himself rejected this hypothesis for reasons to be discussed in a moment; and his last hypothesis turned on a distinction

[1] I should not agree that the brain is in any sense an ' organ of will '. In origin, I admit, it arises as a rudimentary outgrowth concerned essentially with the task of improved motor adjustment: i.e., it is primarily an organ of practical behaviour rather than of cognitive understanding. As an integrative mechanism its chief function is to undertake the complex organization needed for *doing one thing at a time.* To this the mental counterpart is in my view ' attention ' rather than ' will '. As Sherrington puts it, " a ' willed ' act is but the culmination of attention " (*Man in His Nature*, p. 270). However, Dr. Levy might retort that attention at its highest is willed attention, and involves a non-mechanical choice. So here I retain his key-term.

[2] A. S. Eddington, *The Nature of the Physical World*, 1928, pp. 275f. As he explains, however, his chapter on causation and volition had to be largely re-written owing to the fact that in 1927, less than twelve months before publication, " quantum theory definitely entered on a new phase " with the announcement of Heisenberg's principle of uncertainty.

[3] A. S. Eddington, *New Pathways in Science*, 1935, pp. 88f.

between the " uncorrelated or random behaviour " of particles in general as postulated in contemporary physics and the orderly and correlated behaviour of particles in those organized portions of matter which he considered to be in immediate relation with mind.[1] I should fully agree that orderly behaviour is a distinctive characteristic of efficient mental activity and that this is in marked contrast with the physical tendency to ultimate disorder, as expressed in Boltzmann's formulation of the second law of thermodynamics (" the total entropy of the universe is increasing and tends towards a maximum which will correspond with complete disorder among the particles it contains "). But this introduction of ' order ' (or, as I would rather call it, organization) appears to be a description of the *nature* of mental activity rather than of the manner in which mental activity operates on the physical processes within the brain. Hence it need not be an alternative to the second hypothesis but rather a consequence of it.

The chief argument which led Eddington to reject the second hypothesis (which so many other quantum theorists have preferred) may be summed up as follows. He " conjectures that the smallest [neurological] unit in which the physical effects of volition could have their origin must contain billions of atoms; and if such a unit behaved like an inorganic unit of the same mass, the indeterminacy would be insufficient to allow appreciable freedom ". Now the lightest known atom (an atom of hydrogen) has a mass of $1{\cdot}6 \times 10^{-24}$ gm. And if Eddington is using the word billion in its English sense (a million million or 10^{12}), then a billion atoms of hydrogen would have a mass of $1{\cdot}6 \times 10^{-12}$. Now this is very much larger than the mass of a synaptic knob (5×10^{-13} gm). Hence it seems clear that Eddington was assuming that the smallest functional unit would be the nerve-cell. But if, as was argued above, we must regard the synapse or synaptic knob as the smallest functional unit, then his difficulty disappears.

The Physiological Problem. What then does modern neurology make of this suggestion? At this point we must plainly turn from the physicist to the physiologist. Professor Eccles, himself one of Sherrington's ablest collaborators, has discussed the issue at some length. As we have seen, the passage of a nervous impulse involves, not one, but a number of synaptic junctions. And in such a case, as Professor Eccles points out, if these minute influences were exerted at a large number of points consistently and simultaneously, the effects would be " rapidly integrated in the form of a changed spatio-temporal pattern of activity within the neuronal net ". Thus, in the cortex as pictured by contemporary neurologists there unquestionably exists " a mechanism that could effectively amplify these minute effects, provided they form a ' meaningful ' pattern " (i.e., presumably, a self-consistent and convergent pattern), " and are not random ".[2]

We may therefore imagine that in certain circumstances the brain could act as a detector and amplifier of the minute effects exerted by the ' will ', even when no known physical instrument is sufficiently delicate to detect them. But for this purpose certain conditions must be fulfilled.

(i) First of all, a certain number of neurons must already be at or near the critical level of instability. The influence of the ' will ' would then be to increase the probability that some of these neurons will discharge. When the activity of *all* the cortical neurons sinks to a fairly low level, as in states of deep sleep, anaesthesia, and the like, the brain fails to respond to minute influences of the ' will ', just as it fails to transmit impulses coming from the sense-organs unless they are exceptionally intense; and in these exceptional mental states the low level of activity throughout the brain can be readily demonstrated by the electro-encephalogram. However, for the manifestation of consciousness or will only a part of the brain need be active at a critical level: the rest will supply merely background

[1] A. S. Eddington, *The Philosophy of Physical Science*, 1939, pp. 396f.

[2] J. C. Eccles, *The Neurophysiological Basis of Mind*, 1953, pp. 278–79.

activity. And apparently almost any part—almost any set of interrelated cells—will serve.[1]

These unexpected facts have an instructive bearing on the relation between brain and mind. In my view the mistake of Descartes lay, not so much in his dualistic theory of a non-spatial, non-material mind, distinct from a material brain in physical space, but in his proposal to make the two interact at some central pontifical point in the solid spatial mass that makes up the brain. Here " it sits and governs ". His account thus treated the mind as the sole imperial arbiter of a totalitarian state, whereas what we now know of the organization of the brain itself suggests rather that it forms a vast society of living units which must be ruled on democratic principles.

(ii) But secondly, the activity exerted by the ' will ' must itself be an orderly activity. It can only function as an effective modifier of neuronal activity if its own activity exerts a patterned ' field of influence ', to borrow Eccles' phrase. In that case, as he says, the neural output that ensues will be determined by three factors: " (1) the micro-structure of the neural net and its functional properties "; " (2) the afferent input " (including presumably feedback); and " (3) the postulated ' field of extraneous influence ' " exerted by the agency we loosely call the mind or will.[2]

This then is a plausible hypothesis offered to psychology by contemporary neurology. It is avowedly no more than a tentative speculation. But it keeps within the limits prescribed by contemporary physics. The interesting feature is that, instead of the more familiar notion that the mind functions by detecting the activity of the brain, the brain functions by detecting the activity of the mind—and, under certain conditions, by failing to detect or so failing to transmit its influences. And, since the processes involved can best be treated by statistical techniques, the hypothesis becomes of special interest to the statistical psychologist.[3]

[1] As Eccles points out, this is consistent with the clinical evidence that " mind-brain liaison is not interrupted by the excision of a large part of the cerebral cortex or when a large part is occupied by intense activity that excludes liaison, i.e., by a convulsion of alpha-wave activity ".

[2] *Op. cit.*, p. 277.

[3] An extended corollary to this ' detector hypothesis ' might conceivably offer an explanation for some of the anomalous results reported in experiments on telepathy and the like. Let me add, however, that it throws no light on the special Cartesian problem raised by Dr. Smythies and others (*J. Psychic. Research*, XL, p. 395), namely, whether or not the mind itself is extended in space. I would only point out that the fact that certain mental *contents* are spatially extended does not (as he appears to maintain) prove that the *mind* is spatially extended. But, if we accept a relational theory of space itself the question ceases to have much importance. For certain purposes the factorist is of course ready to treat any event (e.g., the fact that n persons simultaneously perceive what they take to be the same object) as specifiable by a number of coordinates (here $3n$) and therefore in the mathematical sense a ' point ' in a multidimensional ' space '. But this is an analytic ' space ', not a concrete space in the sense intended by Dr. Levy and Dr. Smythies.

THE GENERAL AESTHETIC FACTOR. I

To the Editor, The British Journal of Statistical Psychology

DEAR SIR,—The correspondence on 'The General Aesthetic Factor' (this *Journal*, XIII, 87–92) is interesting for what it implicitly raises as much as for the explicit questions put by Mr. Paterson or the statistical evidence offered by Miss Williams and Sir Cyril Burt. I say this because I feel that in his letter Mr. Paterson has not pressed the strong case which he really has, and because the evidence offered is, in part at least, irrelevant to the problem.

Mr. Paterson's question concerns the existence of a General Aesthetic Factor, and how it is to be described if found. Remembering that we cannot get out of a statistical analysis more than we put into it, it behoves us to ask whether the original response-measures, which are used to generate correlations for the analyses, have any relevance to a theory of aesthetic 'appreciation' as opposed to one of 'preference': or in other words, in what sense can the existence of consistent modes of hedonic evaluation, by a culturally homogeneous group of people with minimum intelligence not far short of average, be relevant to demonstrating that aesthetic appreciation is the perception (and presumably the valuing) of sense data in relational form?

There is a minor issue of definition which has first to be settled—whether evaluative feeling-responses in other modes than vision and hearing can be aesthetic. It has, for instance, been maintained by some aestheticians that smell and taste preferences cannot be aesthetic. If we accept this view (on the ground that peculiarities in the phenomenology of taste and smell supposedly prevent the detached non-instrumental character of aesthetic evaluation from occurring), then either we do not demand that the factor analyst shall work with test-batteries covering *all* modalities, or if he insists on demonstrating factors involving these modalities, even when general intelligence and motivation are partialed out, then we could object that what he has found was not aesthetic by definition. For the purpose of argument then we shall accept as aesthetic some responses in all modalities, even including such recherché pleasures as having one's back scratched. The common denominator of the aesthetic tests cited in the evidence lies in the material being accepted by both experimenter and subject as generally desired for aesthetic reasons within the common culture of the two.

The problem is then one of interpretation; and in aesthetics this presumably means either (*a*) that one is testing the theories of some aesthetician where these theories can be translated into operational language, which is more specific than "some people will express a preference for certain definable attributes of objects, and these preferences are themselves better on a metacriterion[1], than other preferences" (which seems to be the most one can get out of a great deal of aesthetic theory) or (*b*) that one is testing a theory based on psychological findings in other fields, such as perception or attitude studies, which can be generalized to aesthetics.

In the correspondence Professor Burt has drawn a parallel with Roger Fry's observations on the peculiar quality of the 'mental attitude' in aesthetic activity. Whether he has confirmed Fry's theory would seem to depend as much on what we accept as measuring operations, when we are investigating common qualities of mental attitudes in a diversity of aesthetic activities, as on the formal similarity between Burt's findings and Fry's assertions[2]. But, if we may be wise thirty years after Fry, a test of different acts of aesthetic

[1] Used in the sense of a criterion to evaluate criteria, *vide* Carnap.

[2] The formal similarity is rather tenuous. Fry is writing of 'a certain positive response', which he 'conveniently labels . . . the specifically aesthetic state of mind'—surely a phenomenologically-defined common denominator of aesthetic experience which we should want to identify (if we identified it with a factor) with the first factor from a battery of tests in which the response-measures were content-analyses of introspective reports having far greater complexity than a mere

evaluation in different contexts with very similar material (stylistically and iconographically) would seem more germane than Burt's evidence ; and we might then discover that the statistically insignificant residuals under the different methods of testing (this *Journal*, footnote 1, p. 91) would be somewhat greater. Incidentally, when comparing the results of aesthetic experiments, the use of the expression ' different methods of testing ' is surely question-begging if interpreted as a statement with implications for aesthetic theory, and not simply as the language of an educational psychologist ; for it implies that there are ' right ' responses and differences in ' ability ' or ' performance ' at tests which in some way reflect the capacity of the individual. Such a statement is not admissible in the context of subjectivist aesthetics, but belongs rather to the implicit absolutism which runs through Professor Burt's (1933) theorizing and his interpretation of his general factor[1].

Oversimplifying somewhat in the interests of brevity we may say that our problem concerns the existence of a general aesthetic factor based primarily on hedonic preference-sorting, and not on judgements of aesthetic merit (a distinction investigated by Morris in *Varieties of Human Value*, 1956), such a factor can be accounted for in two ways : either (*a*) as evidence that aesthetic value exists and can be correctly perceived by an ideal critic to whom we all approximate in varying degrees—the extreme of absolutism— or in a weaker form, that all aesthetic evaluation will tend to take one of a small number of forms in a person exposed to certain material over sufficient time (because of some inherent aesthetic ability which is sufficiently similar in all persons) : or (*b*) at the opposite extreme it might be accounted for by a complete subjectivism in which all value-responses are correct at the time they are expressed, and subjects may tend to agree because of conditioning, group conformity, exposure to selected material, or something like those hedonic shifts which are discussed by Flugel under the heading of ' L'Appétit vient en Mangeant ' (*Brit. J. Psychol.*, XXXVIII, 1948, pp. 171–190).

At one extreme the group factor is interpretable as the expression of that aesthetic perception of which we are all capable in some degree ; at the other extreme it is the artefact of a common culture imposing a conformity of responses on a population of subjects who will, within physiological limits, value any properties, direct or relational, or any object exhibiting such properties, for reasons which are a mixture of the aesthetic and non-aesthetic. It has been suggested[2] that both theories in their extreme form are statements about the use of language, and hence empirically untestable. Professor Burt has not indicated how his statistical demonstration could be part of an *experimentum crucis* between an absolutist and a subjectivist aesthetic theory ; and, until he does so, Mr. Paterson's question remains unanswered. It is, of course, probable that his question is not really one for psychology at all, but for theoretical aesthetics, and that this is disguised by being re-phrased in terms of a *post hoc* interpretation of statistical results.

The contrast between the two explanations of conformity in aesthetic judgements—that of ' culture ' and that of ' innate ability '—has some similarity to the nature-nurture dispute in the assessment of intelligence ; and probably this has misled Mr. Paterson into putting the dichotomy in a rather misleading way, and Miss Williams into suggesting

preference. A choice situation is necessary and sufficient to elicit preferences, but neither necessary nor sufficient to identify that with which Fry was concerned. Burt has quoted it without regard to context. Operationally he has not shown that his tests differentiate between aesthetic choice and non-aesthetic choice except in terms of the material used, a distinction which Fry is anxious to make (*Transformations*, 1926, p. 2, third paragraph).

[1] In *How the Mind Works* (p. 307) we read: " the Venus of Milo would remain more lovely than the Queen Victoria statue in the Mall, the Taj Mahal than the Albert Memorial, though every man and woman in the world were killed by a passing comet's gas ". Apparently not only aesthetic value, but relative values, are absolute in this theory, which is perhaps derived from G. E. Moore's speculations on the fate of a Greek vase buried in the sands of Carthage for all time.

[2] Elton, *Aesthetics and Language* (1954), or Carver, *Aesthetics and the Problem of Meaning* (1952).

that Miss Conway's tests on twins will probably settle the issue. I suggest they will not, because the problem is not so simple as the ordinary nature-nurture dispute ; the measures in the aesthetic tests will have to differentiate between (i) hedonic responses, (ii) responses recognizing formal properties as present and rating them on quasi-aesthetic scales, and (iii) descriptive measures imposed on the material by the experimenters. These appear from the literature to be a hotch-potch of physical judgements and value judgements (' classical ', ' impressionistic ', ' colourful ', and the like). The absolutist theory, if accepted, would permit us to say that the subject whose responses identifying works of art as good are most typical (or heavily loaded with the general factor) will also be the subject whose personal preferences are most closely in agreement with what he identifies as good. The subjectivist theory, on the other hand, maintains that any relation between hedonic responses and judgements that ' art is good (by accepted canons) ' must be culture-loaded, and that the internal consistency of a judge with his own hedonic criteria should be logically independent of any conformity of his preferences with social norms, it would follow that it was also empirically independent, provided we can partial out the effects of previous exposure to material, or of differences in intelligence, or of any other variables likely to induce conformity for extra-aesthetic reasons.

If Mr. Paterson wishes to give vocational guidance on the basis of scores in aesthetic tests, what values of his own is he bringing to the interpretation of the general aesthetic factor ? If popular and hence commercially successful art is practised by persons whose loadings are different from (say) symphony orchestra musicians, would he select subjects whose artistic interests apparently outweigh their talents, by applying the principle of maximizing individual satisfaction, and would he then direct them into commercial art, or would he maximize potential aesthetic value for the community as a whole and then increase the number of unemployed artists whose repertoire is serious but whose execution borders on the inept ? The general aesthetic factor loadings of some of Washburn's subjects (who strongly approved the works of the Pre-Raphaelite Brethren) was doubtless high ; yet I would be reluctant to employ any of them as art gallery directors if they still held the same opinions.

Perhaps one of the protagonists of the ' general aesthetic factor ' as a test measure will defend the ephemeral nature of the test material used in experimental aesthetics, and tell us where ' tachiste art ' will be valued on the classical-romantic bipolar factor, and whether this bipolar would have had the same name if the test material had been composed exclusively of works by 18th century Romantics or 20th century Surrealists. To sum up my objections, although the so-called general aesthetic factor has been shown to exist, either as a fact or as an artefact, nevertheless its status within the context of aesthetic theory is ambiguous, and its validity as a selection criterion is heavily dependent on the culture-loaded and ephemeral material on which it is based. It follows that any psychologist who proposes to use it should always state quite clearly whether he is doing so in order to serve the interests of the applicant in the labour market, or to help in satisfying the man's artistic aspirations, or finally, to preserve and diffuse the aesthetic preferences of the tester himself.

Robert A. M. Gregson.

THE GENERAL AESTHETIC FACTOR (Reply)

Mr. Gregson's difficulty seems largely to arise from a confusion between two kinds of general factor—that obtained by correlating tests and that obtained by correlating persons. Under certain conditions the two may be indirectly related : where the investigation has been planned so that both methods can be applied to the same data, the factor *measurements* obtained by the former are the factor *saturations* obtained by the latter, and vice versa. But here we have to do with two distinct sets of data.

Mr. Paterson's inquiry was concerned with an alleged general factor underlying aesthetic appreciation in all the different fields which it is possible to study. Such a factor can only be confirmed or negated by correlating *tests* for different types of aesthetic appreciation. At first sight it might seem that Mr. Gregson's questions relate to the precise nature of this general factor. But his main argument, if we understand him rightly, turns on the suggestion that the responses to the tests described by Miss Williams and Burt merely express the testee's personal tastes or ' hedonic preferences '; if so, he argues, they could not claim " any relevance to a theory of aesthetic appreciation". But, so far as this suggestion can be dealt with by statistical analysis, it calls for a very different type of approach and indeed a very different type of research from those on which Miss Williams was engaged. Such a question must be settled not by correlating tests, but by correlating *persons*, e.g., by comparing the different orders in which the individuals tested arrange the test-items. Even then the results need to be supplemented by introspections secured from the subjects who take part in the research. Correlations of this type (together with introspections) were given in the book which Mr. Gregson subsequently quotes (*How the Mind Works*, pp. 280f.). But they have little to do with Mr. Paterson's problem.

In some measure the issue which Mr. Gregson raises is, as he himself implies, a matter of definition, and therefore to a large extent merely verbal. Are we justified in applying the term ' aesthetic ' to the general factors we have found ? In point of fact the word was originally adopted as a convenient label. Those who are engaged in the practical tasks which Mr. Gregson mentions—educational and vocational guidance—continually ask the psychologist whether he can supply any convenient tests for ability in drawing, painting, designing, and in the various branches of music. The tests described by Dewar, Williams, and Burt had been constructed for these and similar purposes; and the particular fact of psychology to which we now chiefly wished to draw attention was the existence of a *general* ability underlying skilled execution and judgement in each of these different fields. We needed a term which would be applicable to all these branches of activity. ' Artistic ' (which is sometimes used) tends to suggest visual art to the exclusion of musical and poetic, and so we chose the word ' aesthetic '.

In the chapter on 'The Psychology of Art' which Mr. Gregson quotes, Burt definitely stated that *many* of the responses to the tests there employed (and indeed to many other tests) had little to do with aesthetic feeling in the stricter sense : " the majority of people ", it was said, "offer not aesthetic judgements but personal judgements ". That, however, does not necessarily imply that the tests are not tests of aesthetic appreciation, but merely that many persons, either from lack of endowment or perhaps from lack of training, do not exhibit any marked degree of aesthetic appreciation.

Is there then any evidence that those who score high marks on the tests may be validly described as possessing a general[1] capacity for aesthetic appreciation? Mr. Gregson asks if " the protagonists of the general aesthetic factor as a test measure will defend the ephemeral

[1] By way of validation the results of the separate tests were correlated with teachers' estimates for the *specific* type of ability with which each was concerned. But we suspect that Mr. Gregson would not accept the ordinary teacher as a sound judge even of specific aesthetic qualities. However as the third paragraph of his letter shows, this was not the issue he wished to raise. Both he and Burt (in the passage quoted) are concerned with the nature of aesthetic value in general, regardless of its manifestation in any particular medium.

nature of the material used"; and he seems to suppose that the material was restricted to nearly contemporary works of Western artists and so "culture-loaded". That, however, is scarcely correct. We do not claim to be "protagonists of the factor *as a test measure*" (a phrase used by neither of us); but we may perhaps point out that the visual material used in the tests included reproductions of paintings, statuary, architecture, carving, etc., from a wide variety of epochs and countries. They included paintings by Leonardo, Raphael, Michael Angelo, and Botticelli; such works as Praxiteles' Hermes, the Esquiline Venus, the Venus of Milo, and statuary by Donatello, Michael Angelo, and Henry Moore; and such buildings as the Parthenon and the Taj Mahal. Does Mr. Gregson consider these to be "merely ephemeral"? Moreover, in the whole series of investigations, including the earlier tests carried out over the wireless by Miss Margaret Bulley and Burt and the later investigations carried out by Miss Cancardas on men and women of widely different nationality, the replies obtained came from representatives of almost every available type of culture.

The astonishing thing was that practically all the correlations were positive. This implies the existence of a 'general factor for persons'. As we reported at the time, "though differences in taste and judgement undoubtedly exist, they are far smaller than the heated disputes and discussions on the subject would lead one to suppose". Now when the correlations between the several orders are positive, it is both legitimate and possible to derive a single order, based on a weighted average of the lot, to represent the 'general factor.' We then naturally inquire what kind of individuals—out of all those who took part in our experiment—have produced orders which agree most closely with this ideal or general order? The answer is that they are with few exceptions leading connoisseurs, or eminent performers in the particular field of art with which we are dealing; in other words, they are artists and art critics, musicians and musical critics, poets and literary critics, whom nearly every unbiased person would acknowledge to be each an aesthetic expert in his own particular field. It is this result, as Burt argued in his earlier articles, that justifies us in regarding the general factor as an 'aesthetic' factor.

Mr. Gregson asks whether in our researches we were "testing the theories of some aesthetician . . . or . . . a theory based on psychological findings in some other fields, . . . generalized to aesthetics". Our own answer is—"Neither. We were testing a theory suggested by a finding in separate fields of aesthetics—namely the existence of general factors for artistic (i.e., visual) appreciation, for musical appreciation and for literary appreciation respectively—a finding which we thought might be generalized to the whole field of aesthetics."

Mr. Gregson however supposes we are testing the theory of certain aestheticians who have favoured what he terms an absolutist theory of aesthetic value; and he complains that we have not "indicated how (our) statistical demonstration could be part of an *experimentum crucis* between an absolutist and a subjectivist aesthetic theory". Now in the reports of our statistical investigations there is nothing whatever to suggest that we were attempting to decide between two alternative theories of aesthetic *value*. Our problem was psychological not philosophical; our aim was to decide between two alternative theories of aesthetic *ability*—namely (i) the theory of a general ability entering into all fields coupled with specialized aptitudes peculiar to particular fields, and (ii) the theory of specific faculties for particular fields, with no general ability covering all the different fields —the theory adopted, implicitly if not explicitly, by most of the psychological writers who had previously touched upon the problem.

Now it is true that in the later book to which Mr. Gregson refers Burt certainly discussed the apprehension of value in its various forms—aesthetic, moral, and scientific (popularly identified with 'beauty', 'goodness', and 'truth'), and we both explicitly favoured an absolutist rather than a subjectivist view. The undeniable psychological findings which have commonly been cited in support of a subjectivist view appeared also in Burt's investigations (*op. cit.*, p. 288). But we argued that these merely showed that *some* of the judgements

of nearly all of us and very possibly *all* the judgements of some of us were subjective, and it did not necessarily exclude the existence of an objective component in addition to the subjective.

As Mr. Gregson implies, the controversy over the objective or subjective nature of values is commonly supposed to be purely philosophical. But in our view there is no sharp dividing line between speculative philosophy and empirical science ; and psychology, even more than the other branches of natural science, often has evidence to offer on what were traditionally treated as problems of epistemology or metaphysics. Our own arguments for the objectivity of value were based on a relational theory of perception. The counter-arguments for the subjectivity of value are essentially the same as the familiar arguments for the subjectivity of our other perceptions, e.g., our apprehension of material objects in three-dimensional space. The latter depend on a confusion between two senses of the word perception—i.e., between what we perceive and the process of perceiving it. The latter is undoubtedly subjective, i.e., mental. It involves a relation between a conscious subject and the thing that he cognizes. But this does not imply that the thing cognized must also be mental. And the same reply will hold in the case of the values that we cognize. The fact that a round baize-covered card-table appears to one person oval and to another circular, to one green and to another grey does not prove that the table itself is purely subjective. And the fact that one design seems beautiful to this person, banal to another, and repulsive to a third does not prove that aesthetic value is itself subjective. Equally of course in neither case does our reply prove the opposite view. The objectivity of value, like the objectivity of material things, remains only a hypothesis that can claim the highest probability because of its explanatory power. Thus, to take almost the only argument involving statistical comparisons, the fact that the similarities between the rank-orders submitted by people representing such widely different cultural backgrounds are far greater than the differences seems to favour an objectivist rather than a subjectivist hypothesis. But the arguments we brought forward on *this* particular issue were not statistical, but observational or introspective. We maintained that our experiments demonstrated that what is commonly called aesthetic appreciation is essentially an ability to appreciate " shapes, colours, sounds, and even emotions *in certain relations* ". This not only suggested that the appreciation must be general, i.e., independent of the medium in which the relations were embodied (visible, audible, kinaesthetic, etc.), but also seemed to imply an objective existence for those relations. The use of the word ' beauty ' to describe aesthetic value is perhaps liable to be misleading, especially since, in these modern days, the word has come to be associated with the various cheap and sentimental forms of prettiness. What aesthetic intuition really detects, so we have argued, is the evidence of order in a world that is only partly ordered. But to justify this view at length would lead us far beyond the limits to which the present journal is confined.

E. D. Williams

C. Burt

BOOK REVIEWS

Psychological Twin Research. By TORSTEN HUSÉN. Stockholm: Almquist & Wiksell, 1959. Pp. 153. Sw. Kr. 18.

Dr. Husén's study of adult twins is not only the most recent but also one of the most thorough; and, in view of the renewed interest in mental genetics among British psychologists and educationists, it seems well worth while to examine his conclusions in some detail, and compare them with the results obtained in this country in similar studies carried out on children of school age. The data on which his investigations are based consists of the medical, educational and psychological information obtained for the twins discovered among the young men of 20 called up for National Service in Sweden between the years 1948 and 1951. The original aim, so he tells us, was to study in a large and representative sample " the hereditary-environmental conditions with special reference to intelligence test scores and school marks ". But at an early stage of the inquiry he found it necessary to start by examining, far more closely than previous investigators had done, " the methodological problems of twin research "; and this forms the main subject of the present publication.

The material collected related to nearly 3,000 individual twins. But the analysis here reported is restricted to two main samples: first, a group of 900 pairs, who constitute the basis of the main investigation; and secondly a ' control sample ' consisting of 50 pairs, living in the Stockholm area, who were subjected to a more intensive study in order to check the validity of certain of the diagnostic methods employed. The tests used were mainly verbal, and were applied to groups. They consisted of (i) Synonyms, (ii) Concept Discrimination, (iii) Directions (substituted after 1949 for the original Number Series), and (iv) Matrix. The statistical analysis is unusually thorough.

In general the results thus obtained with adult twins fully confirm the conclusions drawn from studies of children in the London surveys. To begin with, it is noted that in both the physical and mental development the average level of twins falls below that of the general population. In height and weight the difference was about 0·20 S.D.; and, as with the London twins, the inferiority was most marked in the case of monozygotic pairs. With the intelligence tests the average of the twins was 4 I.Q. points (about 0·25 S.D.) below that of the rest of the population. Dr. Husén reports that " there were nearly twice as many twins as single-borns with test scores below I.Q. 75 ". Similarly in London schools there was a decidedly greater proportion of twins in special schools for the mental deficient than in the ordinary elementary schools. The differences Dr. Husén attributes chiefly to disturbances during the pre-natal period and at birth. This also agrees with the evidence as to causal factors obtained from the case-histories in our own investigations.[1]

For height and weight the intra-class correlation amounted to 0·89 and 0·81 with the monozygotic pairs and 0·59 and 0·56 with the dizygotic group. The London figures were 0·95 and 0·91 for monozygotic pairs and 0·47 and 0·58 for dizygotic.[2] Dr. Husén notes, as we had done, that height is affected more by ' heredity ' and less by environment than weight. With the intelligence tests the correlations for the monozygotic pairs was 0·90 and for the dizygotic 0·70. Our figures with group tests were 0·91 and 0·54 respectively.[3] The fact that his figure for the dizygotic twins is somewhat higher than those found in earlier studies he himself attributes to the fact that his batch was confined to one sex only: we should be inclined to attribute it partly to the fact that the persons tested were older and the tests employed more heavily weighted with verbal items. As Dr. Husén

[1] Cf. C. Burt and M. Howard, this *Journal*, IX, 1956, pp. 123f. and refs. Mr. Sandon reports similar results (*ibid.*, XII, 1959, p. 135.)

[2] C. Burt, *Brit. J. Educ. Psychol.*, XXV, 1953, pp. 168; cf. J. Conway, this *Journal*, XI, 1958, pp. 187. (The figures for monozygotic twins here include both those reared together and those reared apart.)

[3] Cf. Burt and Conway, *loc. cit. sup.*

observes, " the total variance, which is made up of hereditary variance and environmental variance (apart from errors of measurement), is the same for both standing height and test scores. But," he continues, " we know from previous investigations that environmental influence of height is rather small; hence the coefficient 0·89 for height should be considered almost entirely as an expression of hereditary influences." However, scores obtained with verbal tests are bound to be influenced in some degree by the cultural environment; and consequently it is admittedly rather rash, on the basis of mere analogy and in the absence of further evidence, to draw the same conclusion in regard to the influence of hereditary factors on test scores as we have drawn in regard to height.

How then are we to proceed? " By comparing the intra-class correlation for a relatively large sample of monozygotics and dizygotics reared apart, with corresponding values for such twin pairs reared together, we should," he contends, " be able to tackle these problems more readily: in this way the concrete significance of environmental difference could be ascertained." Unfortunately, however, in his monozygotic group comparatively few partners had been separated for a year or more. In the undetermined group, we are told, " there were a considerable number of pairs reared apart during longer periods or even from birth, but in these cases it was apparently impossible to carry out a ' similarity diagnosis ' or a more intensive study ". Nevertheless, his results, so far as they go, seem in full agreement with the conclusions drawn from separated monozygotic twins in the British inquiry.

The correlations for school marks averaged 0·77 with the monozygotic twins, but only 0·52 with the dizygotic. In six cases the monozygotic twins were sent to schools differing in level by " two or more steps " (e.g., primary and lower secondary with leaving certificate), and with these the average difference in the intelligence tests amounted to 6·5 I.Q. points. This is what we might expect from the nature of the tests employed, and confirms the view that the ' intelligence ' tests used were in fact appreciably influenced by schooling. Among the London cases there were four in which only one monozygotic twin had been allocated to a secondary school of the ' grammar school ' type; and here the average difference in the scores for the *group* tests amounted to 4·5 I.Q. points. With individual tests the difference was statistically insignificant.

Of the other comparisons made by Dr. Husén one of the most interesting relates to their handwriting. It is often said that " identical twins write almost identical hands ". Certainly when the similarities are determined by a standardized scale of handwriting (though not when determined by impressions of general similarity or by the assessment of specific characteristics) most investigators report a greater resemblance for monozygotic twins than for dizygotic. Thelen, for example, who used the matching procedure found with German twins (aged 14 or over) a coefficient of 0·82 for the monozygotic but only 0·57 for the dizygotic; however, with twins of school age the difference between the two groups was much smaller.[1] In a group of 48 pairs Dr. Husén found for the 25 monozygotic pairs a ' mean similarity rating ' of 2·93 and for the 23 dizygotic pairs one of 2·73. With such small samples, however, the difference was non-significant. He concludes that " intra-pair similarity in handwriting does not differentiate between the two types to anything like the same extent as intelligence test-scores, school marks, or anthropometric measurements".

One of the most interesting features of the whole research is the study of the various clues available for determining whether or not a given pair of twins is monozygotic or dizygotic. Hitherto the most instructive investigation of this methodological problem has been that of Essen-Möller.[1] To obtain a criterion he selected two distinct groups : (i) those twins where the records had reported only one chorion at birth, and treated this as a safe indication of monozygosity; (ii) those where either the sex or the blood group or

[1] E. Thelen, ' Zuordnungsversuche an Schriftproben von Zwillingen ', *Zeitschr. f. Psychol.*, CXLVII, 1939, pp. 215f.

[2] Cf. E. Essen-Möller, ' Empirische Ahnlichkeitsdiagnose bei Zwillingen ', *Hereditas*, XXVII, (1941), pp. 1f.

both was different, and treated this as a safe indication of dizygosity. He then calculated the relative number of cases in which a classification based on various readily accessible clues agreed or disagreed with this basic classification. This enables him to express the value of each clue in terms of a probability coefficient. He then derives a formula for estimating the accuracy of a diagnosis based on all the available and independent clues, viz.

$$p=\frac{1}{1+\left(\frac{D_1}{M_1}\cdot\frac{D_2}{M_2}\cdots\frac{D_i}{M_i}\cdots\frac{D_n}{M_n}\right)P},$$

where p (the proportion of correct determinations) indicates the accuracy of a poly-symptomatic diagnosis based on n symptoms, M_i and D_i the proportions of monozygotic and dizygotic twins showing the ith characteristic, and $P=$the ratio of dizygotic to monozygotic pairs (usually about 5 to 3 in the case of same sex twins). The structure of the formula becomes intuitively intelligible if we consider cases in which there is only one symptom: we then have $M/(M+D\times P)$; and this $=1$ when $D=0$.

Dr. Husén's ' control investigation ', as he calls it, was based on 50 pairs—26 being monozygotic and 24 dizygotic. These were subjected to a detailed examination (including a standardized interview) lasting for about two hours. His methods to a large extent follow those of Essen-Möller. However, even when it was possible to discover the hospital in which the twins were born, the information available seldom indicated whether or not there was only one chorion at birth; and consequently his criterion for monozygosity was a ' final diagnosis ' based on all the measurements and general information available. Tests for four uncorrelated blood group systems (ABO, MNS, Rh, and P) were taken by the staff of the Institute of Genetics, and analyses were obtained from the National Chemico-Legal Laboratories. It appeared that, if a pair belongs to the same blood group in all four systems, the probability that they were dizygotic was only 7 per cent and that they were monozygotic 90 per cent, the MNS and Rh systems being the most informative, since in these the dizygotic pairs were dissimilar in 12 out of 22 cases. A diagnosis based on 10 monozygotic characteristics gave probabilities of 76 and 18 per cent. Among the other characteristics the frontal hairline, the colour of the hair, and the complexion was nearly always the same in the monozygotes (the exceptions being less than 9 per cent). They were also the same, however, in about half the dizygotes. Of the other characteristics nose, lips and teeth were strongly suggestive, but shape of ears (contrary to the common view) proved far less reliable. The study of the portraits is also instructive. With full face photographs the 22 monozygotes were given an average dissimilarity rating of less than 20 and all except 4 of the dizygotes a rating of more than 20. One obvious clue, however, Dr. Husén has apparently omitted, which other investigators have regarded as highly suggestive, namely, the comparison of fingerprints.

According to the replies given by the monozygotes 28 per cent had sometimes been mistaken for each other by their parents, 19 per cent by their siblings, 85 per cent by their friends, and 89 per cent by their teachers. Dr. Husén therefore believes that " we can depend to a considerable extent " on inferences drawn from such replies. With London children we found that the twin's own intimate playfellows rarely made mistakes, and that mistakes were quite exceptional in a normal home circle. At school and elsewhere girls were confused more easily than boys—chiefly because among the girls identical twins tend to adopt the same dress and hair-style, and secretly enjoyed the confusion so caused. Apart from the recorded details of the birth, we found the best single clue was the general impression of the experienced examiner: when further evidence was subsequently available it appeared that, if a pair was classed as ' identical ', the diagnosis hardly ever proved wrong; but a few errors were discovered where the pair had been classified as ' dizygotic '. With this Dr. Husén's results concur. About 10 per cent of the monozygotes were, he states, wrongly judged to be dizygotes; and he thinks that similar mistakes may have occurred in earlier

inquiries so that many monozygotic pairs have been wrongly included in the dizygotic group. " Hence," he says, " the part played by environmental variance has been exaggerated."

In nearly all attempts to disentangle environmental influences from genetics there is one troublesome circumstance which is too often disregarded, namely, the correlation or ' interaction ' between the two. Most investigators have too often assumed that the quality of the environment and the amount of environmental variance are the same for all members of the same family, whether monozygotic twins, dizygotic twins, or ordinary siblings. Common observation, however, suggests—and systematic comparisons[1] have fully confirmed—that environmental influences will show greater similarity in the case of twins than with other siblings even when of the same sex. ' Identical ' twins are notoriously more closely attached; they go about together; they cultivate same interests; and they share in the same enterprises. Often there may be a tacit agreement that one of the two shall act as spokesman, but this does not prevent their opinions and decisions from being joint. These are facts familiar to every school teacher from personal observation of his own pupils. Dr. Husén has taken up this point in studying his group of adult twins. His method was to address a formal questionnaire, suggested by the findings of earlier investigators, to the twins themselves. He found that the ' attachment score ' (based on a quantitative analysis of the replies) " was of the same order of magnitude in both the DZ and the MZ groups ", and " in both groups the tendency towards attachment is lower than during their childhood. . . . What differentiates the two twin groups most is the striving to be dissimilar manifested by the DZ twins."

Dr. Husén's whole research is one that might well be emulated by investigators in this country. Primarily, as he has emphasized throughout, it is a study in methodology; and, as such, will be invaluable to those planning similar inquiries. But, quite apart from the details, there are several broader lessons that the genetical psychologist can readily draw.

(i) First of all, the analysis of data already available, such as test-scores or school marks, though useful in a pilot-inquiry and suggestive as a starting-point, is inadequate for purposes of conclusive research. The ordinary group tests of ' intelligence ' are too deeply affected by educational and other environmental conditions to throw much light on genetic differences. Far more trustworthy methods of assessment are needed, e.g., individual tests, depending as little as possible on the cultural background, planned and applied specifically for the research in hand, preferably by two independent investigators working in cooperation, and checked whenever possible, independently by the observations of those who already know the examinees, such as their parents and teachers.

(ii) As Dr. Husén himself has argued, by far the most convincing method of distinguishing environmental effects from genetic is to compare cases in which the ' identical twins ' have been brought up in homes of a widely different type. The prejudice against separating twins is not so strong in the case of new-born infants, and, as Burt has pointed out, there are far more cases of separated twins than is commonly realized. With the cooperation of officials, and under a guarantee that confidential information will be duly respected, it is by no means difficult as time goes on to collect and examine a large number of cases.

(iii) It is important that accurate records should be preserved of the details relating to deliveries of twins so that future investigators are not confronted with the difficulties Dr. Husén and others so frequently experience in deciding which are monozygotic and which are not.

(iv) Dr. Husén is undoubtedly right in giving the highest priority to the study of methodology—i.e., of the most appropriate experimental designs and the effective statistical

[1] Cf. H. von Bracken, ' Mutual Intimacy in Twins : Types of Social Structure in Identical and Fraternal Twins ', *Character and Personality*, II, 1934, pp. 293f. For typical anecdotes illustrating ' common observation ' see Galton, *Inquiries into Human Faculty*, pp. 157f.

techniques. He himself (again quite rightly) starts with Fisher's intra-class correlation coefficient rather than with Pearson's product moment coefficient. But it may be remembered Fisher himself went on to express the intra-class correlation in terms of the variances due (*a*) the hypothetical contributory factors, i.e., environment and heredity, and (*b*) to the observable components, i.e., the variance within families and the variance between families. This leads to a method of estimation which Burt has called ' the factorial analysis of variance ', developed on the assumption of a multifactorial Mendelian inheritance. How far Fisher's techniques need to be modified or amplified in the light of those since developed by Mather and Sewall Wright are further problems in methodology calling for practical trial.

(v) From Dr. Husén's work it also seems clear that researches on mental genetics can be carried out far more effectively and far more readily with children than with adults. Children are easier to test; they can be interviewed more readily and more fully and the relevant information is more readily accessible. Comparative data for adults is, as Dr. Husén's investigations demonstrate, highly desirable; but this, it would seem, could best be secured by following up cases that have been first studied during the school period.

(v) Owing to the supreme importance of the questions involved the inquiries should be carried out on an official basis. Large numbers of subjects and large masses of accurate information are essential. And this of itself requires time, and money, and trained personnel. The unofficial and amateurish efforts of isolated research-students is likely to add little to existing knowledge. But to secure adequate cooperation it will be necessary to arouse public interest in these branches of research as well as in those more spectacular branches of scientific research which have hitherto chiefly attracted official and popular attention.

J. Conway

Chance, Skill, and Luck. By John Cohen. Harmondsworth: Penguin Books, 1960. Pp. 201. 3*s.* 6*d.*

This is an attractive book with an unattractive title. As he explains in his prefatory chapter, Professor Cohen's real aim is to analyse those modes of behaviour which involve that superlatively human activity we call "guessing", or, in other words, conduct that entails " taking risks ". His treatment, he tells us, will be psychological—that is partly experimental, partly statistical, partly introspective, but not behaviouristic in the sense of being essentially neurological. The neurological model for man, he tells us, was set up by psychologists not by neurologists. " Its fundamental weakness is its impersonal character "; and risk-taking is an eminently personal affair.

He begins with a brief historical introduction. This leads him to trace the parallel between such ancient social institutions as casting lots, trying by ordeal, and resorting to oracles, and other forms of divination, and the modern individual customs of trusting luck, looking for omens, and reading inferences from horoscopes as offered in the newspaper. The scientific study of the subject is of comparatively recent origin. " Un problème posé à un austère janseniste par un homme du monde ", says Poisson, " a été l'origine du calcul des probabilités." The " problem " arose out of certain bets placed by an inveterate but thoughtful gambler. But the " austere jansenist " considered only the rational, not the emotional or personal factors, and concentrated solely on the mathematics. Indeed, as Burt has remarked, " from that day to this the *psychology* of gambling and betting has never yet been properly studied ": the gambler, he says, as assessed by those who know him, is " of an extraverted rather than introverted type and of a sanguine or at least superficially euphoric type : half his time he lives in a world of wish fulfilment ". Temperamentally he shows many striking resemblances to the typical delinquent, and " though most gamblers may not be criminals, nearly every criminal is in outlook an habitual gambler ". " While the pessimistic introvert trembles before a risk of 1 in 200, the optimistic extravert—and that includes many criminals—will ignore a risk of 1 in 3 or 4 if it runs counter to his wishes, but treat it as a certainty if it would favour what he desires." Similarly Professor Cohen notes that a chance of 1 in 7,000 for winning a prize is regarded

as well worth accepting by the hardened gambler ; but the same chance of dying as a result of crossing the road would be considered negligible by nearly everyone. There is one further point of similarity between gambling and crime. Since the war, gambling, like delinquency, has considerably increased. " In 1910 the turnover of the gambling industry amounted to about £100,000,000 ; by 1938 it had risen to approximately £380,000,000 ; by 1947 to more than £700,000,000 ; and it is still increasing."[1]

Professor Cohen is interested in the subject from the standpoint of general psychology rather than of individual psychology; and logically no doubt the former aspect should be investigated first. He views it as a problem in probability; and his mathematical procedures are largely based on those with which psychological students will already be familiar from their textbooks on statistics. But he distinguishes two types of probability—objective probability (which the statistician investigates) and subjective probability, or, as he now prefers to say, psychological probability (which the psychologist investigates). I myself, however, would venture to suggest that the real subject should rather be called ' the intensity of conviction ' : " the variable to be assessed and studied is not the strength of a certain type of probability, but the strength of certain beliefs ". Professor Mace in his Foreword gives a neat illustration. Just before the last general election a celebrated statistician told an eminent man of business that his analysis of the probabilities strongly indicated a win for the Conservatives. After the election they met again, and the business-man reported : " I made a handsome profit by buying Steels: and you? " "Alas," said the statistician, " I lacked the courage of my conviction." " The odd conclusion follows," adds Professor Mace " namely that the statistician did not really *believe* what as a statistician he knew to be true—that the *probability* of a Conservative victory was very high." In short, as he puts it, the ultimate effect of these investigations should be to " restore to psychology the concept of belief, which has almost disappeared from the textbook ".

CHARLOTTE BANKS

Numerical Methods of Curve Fitting. By P. G. GUEST. Cambridge: University Press, 1961. Pp. xiv + 422. 80*s*.

The author's aim is to give a comprehensive account of methods of combining or reducing sets of observations. Part I deals with observations of a single variable. Under this head he discusses the general theory of frequency- and probability-distributions, the postulates leading to the arithmetic mean (least squares, minimum variance, and maximum likelihood), the properties of the normal curve and of the commoner discrete distributions, and the relevant statistical tests. Factorial design and analysis of variance are deliberately omitted. We then turn to cases where the observations are concerned with two variables. Part II describes the fitting of straight lines and the theory of regression equations. Special attention is given to the problems arising when the observations are not equally spaced, and when they are not of equal weight, and finally when the independent variable is itself subject to error. Part III, which occupies nearly two-thirds of the volume, deals with the fitting of polynomials and of non-linear functions which are not polynomial. This leads to a discussion of the general regression equation and problems involving functional relations between several variables. Free use is made of matrix notation to express and deduce the more complicated results, but a knowledge of only the simplest matrix operations is assumed.

Throughout the book full derivations are given for the formulae used. Each method is illustrated, not only by a worked example, but also by a complete calculating scheme, so

[1] C. Burt, *The Contributions of Psychology to Social Problems* (Oxford University Press), pp. 61f.; cf. *id.*, *The Young Delinquent*, p. 152. Professor Cohen's figures are somewhat smaller; but he observes that the estimates can only be very approximate. In any case he fully confirms the remarkable increase. In his preface Professor Mace also suggests that the psychological study of gambling may have an important bearing on the problem of crime.

that the beginner can proceed along well tried paths. The schemes presented for the more complex procedures are designed primarily for a desk-computer. Tables needed for the practical calculations are printed at the end of each chapter. A couple of alternative procedures are given for calculating orthogonal polynomials where high speed automatic computers are used, but most of the schemes are suitable for either method of computation. A full bibliography of over 200 numbers is appended; and the whole book is admirably produced.

The examples, while highly interesting in themselves, are restricted to physics ; and the types of observations discussed are those most frequently encountered in the physical sciences. However, the author hopes that the book will also be useful to those engaged in the biological sciences. Since many of the techniques presented here find no place in the usual statistical textbooks, it should also prove an invaluable work of reference for research workers in psychology. CYRIL BURT

Attitude Scaling. Edited by J. DOWNHAM. London: The Oakwood Press, 1960. Pp. iv+76. 15*s*.

In this country psychological researches on ' attitudes ', as they are now called, started nearly fifty years ago with studies of the attitudes of school children towards their school, their teachers, and most frequently of all towards the subjects of their curriculum. The concept of ' attitudes ' was based largely on McDougall's theory of ' interests ' or ' sentiments '. These are all alternative names for much the same thing, namely, " those complex conative dispositions which are concerned largely with valuation rather than cognition, and which determine in a more or less consistent fashion the subject's behaviour." The earliest studies treated the problem as essentially qualitative. Later the concept was somewhat simplified, and attempts at quantitative scaling were introduced. As a rule, the procedures used were based either on simple ranking or on one of the traditional ' psychophysical methods '. Finally, a fresh attempt was made to deal with the more complex aspects of the subject by the application of factor analysis. With the founding of the National Institute of Industrial Psychology this type of research spread into the vocational and commercial fields; and about 1930 the publication of several highly instructive American studies, first by Thurstone and his co-workers, and then by Likert, Cantrill, and many others, aroused a lively interest in the statistical techniques.

The present volume contains a series of papers presented at the Second Conference held by the Market Research Society, which is also responsible for the publication. The nine contributors are British investigators, all of whom are, or have been, engaged in research on personal or social attitudes. Most of the discussion is concerned with methodology—the measurement of attitudes by formal interviewing and the choice of statistical techniques for analysing attitudes and making reliable predictions. Norman Squirrell, Market Research Officer for a well known firm, writes an introduction to the series. Of the papers that follow, one of the most useful—' A General Introduction to Attitude Scaling Techniques '—is by J. B. Handyside of the National Institute of Industrial Psychology. D. L. Lamberth, Operations Research Manager at Messrs. J. Lyons and Co., briefly discusses the place of attitude assessments in what is termed ' operations research '; Joseph Trenaman, who at the time of the Conference was Further Education Officer at the B.B.C., gives an admirable account of Guttman's ' scalogram ' technique; R. P. Kelvin, of University College London, describes how rational, irrational, and unconscious factors can influence the attitudes of consumers; and J. D. Downing describes an instructive experiment planned to study the attitudes of potential customers towards two different brands of the same commodity.

The chief interest of the whole collection springs from the fact that it arises out of the co-operative efforts of scientific investigators who represent widely different fields of application and approach them from widely different standpoints. It should serve as a valuable introduction for younger British workers, and might well be followed by a further symposium dealing not so much with methods as with typical results. I. E. ROBERTS

Introduction to Econometrics. By Oskar Lange. London: Pergamon Press, 1959. Pp. 384. 35*s*.

This book is based on a series of lectures given by Professor Lange in the University of Warsaw and translated from the Polish by Eugene Lepa. The synthetic word ' econometrics '—modelled on Galton's term ' biometrics '—was coined by Ragnar Frisch, the Norwegian statistician and economist, in 1927. And, as biometrics soon became largely independent of biology, so econometrics seems to be developing a similar independence of traditional economics. According to the present author it has three main aims: first, to determine economic trends and so to forecast future situations, notably the business cycles which he regards as a special feature of capitalist societies; secondly, to investigate the problems of demand and supply, that is, of ' market research ' in the broadest sense; thirdly, to lay the bases for planning or programming. Each of these fields is discussed in turn in the present volume—the first most briefly, and the last in considerable detail. "Economics", it has been said, " is itself a branch of applied psychology, since the phenomena with which it deals are the result of human discrimination, valuation, and motivation ". Hence, just as the economist has much to learn from psychology, so the psychologist could learn a good deal from the methods and results of the economist as set forth in this volume.

" The first econometric research ", we are told, was that which led Pareto to formulate the law of income distribution that goes by his name. In its cumulative forms the law is expressed by the simple equation, $N = C/x^{\alpha}$, where N is the number of persons whose income exceeds x units, C is a constant, and the exponent a—a so-called ' coefficient of elasticity '—measures the inequality of the incomes (Pareto suggests it varies about 1·5 or rather less). Or, differentiating, we can write $y = aC/x^{a+1}$, where y denotes the proportionate number of persons having an income of $x \pm \frac{1}{2}dx$ units. Professor Lange takes two examples of income distribution, one for Britain in 1911, and a second for the U.S.A. in 1919; and shows that, particularly in the case of the British data, the fit is quite good. American economists have maintained that the equation really expresses " a natural law of the distribution of human abilities "; and Professor Lange himself points out that a similar J-shaped distribution results from the effects of a series of examinations taken during successive years of study: the largest proportion pass the first examination, a smaller proportion the next, and so on. However, he believes that, as applied to incomes, the curve is really the effect of the unnatural type of successive screening that applies chiefly in capitalist countries. The present distribution of incomes among employees in Poland, he finds, does not fit Pareto's formula, but is rather " reminiscent of the right-hand half of the normal curve ". And he quotes other investigators who report that, with relatively homogeneous social groups, the distributions nearly always approximate to the normal, though the curve is usually truncated asymmetrically at either end. He is, however, unable to offer any convincing explanation of the curves obtained or of their divergences and odd anomalies; and he appeals, quite rightly, for further research.

Now, as Burt long ago pointed out in discussing this and other equations, the distribution of abilities is approximately normal, as we should expect from the multifactorial theory of inheritance. But a very similar equation has been derived by Marshall and his followers to express the relations of demand and supply when what he calls the ' elasticity ' is constant ($d = C/p^{a}$, where d denotes demand and p price). This suggests that motivation, as well as ability, is involved. Pareto's law, according to Burt, expresses not the distribution of ability, but the distribution of output, and output depends on *both* psychological variables (' Ability and Income ', *Brit. J. Educ. Psychol.*, XIII, 1943, pp. 93f.). Their influence on one another is expressed not by adding the influences, but by multiplying them (cf. Table IV, *loc. cit.*, p. 96). If the motivational factor were nearly the same for every individual in the sample, the distribution of output would be approximately normal; otherwise it would approach the asymmetrical and J-shaped distributions such as are described by Professor Lange.

By far the longest chapter, nearly half the volume, is devoted to the theory of ' programming '—a term that is used to describe " economic planning when the planning is not on the scale of an entire country ". As Professor Lange points out, the theory of the subject developed in the U.S.A. during World War II in connection with problems of supply for the armed forces and with the organization of the forces themselves. It is a topic to which he himself has made important contributions, particularly in that branch of the subject known as ' input-output analysis '. The problems emerging he treats very largely by the aid of matrix algebra, deducing ' characteristic equations ' similar to those involved in ' factor analysis by weighted summation '. This part of the book is naturally of greater interest to economists than to psychologists. Nevertheless, in their formal aspects the questions that arise are very similar to many which the psychologist encounters. And accordingly psychologists as well as economists should be very grateful for the enterprise which has resulted in the translation and publication of this highly illuminating volume.

I. E. Roberts

Social Mobility in Industrial Society. By S. M. Lipset and R. Bendix. London: Heinemann. California: University of California Press, 1959. Pp. xxii + 309. 30*s*.

Dr. Lipset and Dr. Bendix deal with three main problems: first, social mobility in general as a characteristic of human communities; secondly, social mobility in one particular group, treated as a representative community, viz., the East Bay section of metropolitan San Francisco; and thirdly the causes, dimensions, and consequences of social mobility. The more general discussions are based on a wide survey of the relevant literature.

The authors' opening chapter leads up to what they rightly describe as " a startling conclusion ", namely, that the degree of social mobility seems to be much the same in nearly all industrial societies at nearly every period of time. As they point out, the opinion hitherto most frequently voiced by American writers is very different from this: the United States, it is assumed, form an ' open ' society with considerable mobility, while most European countries (England, France, Germany, etc.) have societies that are ' closed ', so that children of workers are more or less forced to remain in the social position of their parents, and children from the upper classes remain in the upper classes. A parallel view obtains over here. As regards this country there was in the past, so most British educational and social psychologists apparently believe, hardly anything approaching social mobility until quite late in the nineteenth century; but during the past 50 years or so, owing to improved standards of culture among the working classes, and the great increase in free places at secondary or grammar schools,and above all to the steady weakening of the barriers separating one class from another, it has rapidly become very much easier for children of the so-called lower classes to rise into the higher. Perfect freedom, it is admitted, has not yet been attained; but the abolition of the 11-plus examination, the general introduction of comprehensive schools, and other measures of educational and social reform, will in the end " virtually banish class differences". And the same writers commonly point to America as " a model country whose educational system we might do well to emulate " and to certain other backward countries in Europe as even more immobile than ourselves.

What then is the evidence of the facts so far as they are now available? Mobility, as our authors point out, is of two main types—intra-generational and inter-generational. Researches on intra-generational mobility seem to be comparatively few. In a British survey carried out in 1949 by Geoffrey Thomas it was found that of persons whose first job had been of a manual type as many as 21 per cent had, by the time of the survey, risen to an occupation of a non-manual type; and similarly of those who started with a non-manual job 23 per cent had dropped to a humbler manual employment. Similar investigations have been carried out in America and Japan, with similar results: the chief difference—by no means a large one—is that in these other countries it is commoner for people to rise to higher jobs than to fall to lower jobs.

Studies of inter-generational mobility are more numerous. Accordingly for purposes of their main comparison Lipset and Bendix define mobility as the proportionate degree to which sons of manual workers rise into non-manual classes or vice versa; and their evidence is based on quantitative data collected in various countries since the end of the last war. In view of the recent discussion of this and kindred problems in the previous issues of this *Journal* it is perhaps worth while bringing together the actual figures given in their tables and diagrams.

TABLE I. DEGREE OF SOCIAL MOBILITY IN VARIOUS INDUSTRIALIZED COUNTRIES

	Upward Mobility	*Downward Mobility*	*Total Mobility*
Urban Only			
United States A	35	25	30
United States B	31	34	
France	35	18	27
Germany A	27	38	31
Germany B	30	20	
Germany C	26	28	
Sweden	31	24	29
Switzerland	44	13	23
Japan	33	21	27
Urban and Rural			
Great Britain	20	49	29
Denmark	22	44	31
Italy	8	34	16

Note.—In the first nine surveys the figures in the first two columns give the percentages of the total number of sons of manual workers who have risen to non-manual occupations and of sons of non-manual workers who have taken up manual occupations. In the last three the distinction is between occupations of high and low prestige, which is not always identical with the distinction between non-manual and manual work. In particular, in Denmark and Italy, and perhaps even in Great Britain, the inclusion of farmers, etc., may very possibly have exaggerated the amount of downward mobility. Accordingly an attempt is made to derive a single overall index of vertical mobility by adding the raw figures on which the first two columns are based and then reducing the result once more to a percentage.

It is unfortunate that the methods of classification are not always based on the same criteria. But one might expect this to exaggerate rather than diminish the differences. Nevertheless, the data seem to give a fairly reliable indication of the fluidity of urban populations. Except perhaps for Switzerland and Italy the amount of social mobility (as defined by the authors) is much the same in all the countries studied as in the U.S.A.

The main conclusion is not entirely new. In his book on *Social Mobility* (1927) Pitirim Sorokim undertook a detailed examination of the data then available, and came unhesitatingly to the conclusion that, contrary to the prevalent view, there was no greater mobility in the United States than in the industrialized countries of Europe. Similarly Professor Glass, in the investigation discussed in an earlier issue of this *Journal* (X, pp. 10f.), compared the amount of mobility at various periods from the end of the last century and during the earlier decades of the present century, and could find little sign of an increase in spite of the popular impression to the contrary (*Social Mobility in Britain*, 1954).

If this were a complete and reliable account of the matter, it would provide a strong refutation of the views held by those educational and social psychologists to whom I have

already referred: for it would seem to imply that, however much we improve the education of the underprivileged and however much we may succeed in abolishing class distinctions, the amount of upward mobility would remain unchanged. Nevertheless, the rather crude method of assessing mobility on which the investigations summarized in Table I are for the most part based is in my view both incomplete and unconvincing. The division of the population into manual and non-manual is only one of the many class distinctions that it is possible to draw. Moreover, in the majority of the investigations cited the term ' non-manual ' includes relatively humble white collar jobs such as those of routine clerks as well as the upper professional classes. But when the working class boy is seeking to rise to a higher social level his aims soar much further than that of a non-manual worker on an office stool. In most cases what he hopes to do is first to win admission to a grammar school, and then eventually to achieve either university education and a place among the professional classes or else its equivalent in the world of commerce or industrial management.

Now when we consider mobility into and out of the professional classes the story is quite different from that which the two American authors describe. Take, for example, the social origins of higher civil servants in this country (those above the rank of Assistant Secretary): in 1929 only 6·6 per cent had fathers in the Civil Service; in 1939 the proportion had risen to 9·5, and in 1950 to 15·6.[1] Much the same chance can be traced in the history of the medical, legal, and teaching professions. Moreover quite large variations are found when instead of comparing times we compare places. In Britain, France, Germany, and Sweden a smaller proportion of civil servants and professional men are drawn from the manual classes than is the case in America, but a greater proportion among the members of the legislature and the cabinets. This may be partly related to the fact that in America far more youths attend universities than in Europe, while labour parties as such have a remoter political role. But even here we must be careful not to generalize too rashly. If we turn from the higher professional strata to the analogous strata in commerce and industry, the variations are far less striking. As long ago as 1912 an inquiry undertaken in the British cotton industry by Professor Sidney Chapman and Fred Marquis (now Lord Woolton) showed that over two-thirds of the owners, directors, and managers had begun their careers as manual workers or routine clerks.[2] And a series of investigations carried out in various countries indicates that the social backgrounds of American and European ' business élites ' is very similar. Mrs. Floud too has found that today the proportion of male teachers drawn from the manual classes is actually a good deal smaller than it was before the war ; and studies of members of legislatures and cabinets indicate that in Europe far more politicians are drawn from the lower social classes than is the case in America.

One other restriction affects the figures I have quoted. They relate solely to social mobility among males. During the last seventy years or so there has been a vast extension of the number of non-manual occupations open to women—commercial, clerical, and even professional; and different countries vary enormously in the extent to which women are able to take advantage of these new opportunities.

But the question that will chiefly interest readers of this journal will be the extent to which social mobility is related to the psychological characteristics of the individuals and particularly of the children. According to Dr. Lipset and his colleague, "in a bureaucratized industrial society the main source of occupational achievement is educational achievement". Certainly this is very true of the United States, where, we are told, " a college education has become an almost necessary requisite for any position of responsibility "; in Britain a university degree is considered essential only for entrance to one of the professions. Now educational achievement is dependent on the individual's intelligence. Hence we may suppose that the limitations of an individual's inborn capacity are likely to limit his powers

[1] R. K. Kelsall, *Higher Civil Servants in Britain from* 1870 *to the Present Day*. 1950. Pp. 150f.

[2] ' The Recruiting of Employing Classes from Ranks of Wage Earners ', *J. Roy. Statist. Soc.*, LXXII, 1912, pp. 293f.

of rising to a high position. On the other hand, the mere possession of a high measure of innate ability does not of itself guarantee a high occupational level: this is amply shown by follow-up studies of junior county scholars in this country and of gifted children in America.[1] Health, motivation, social advantages, all play an appreciable part.

Dr. Lipset and Dr. Bendix conclude that social mobility may be regarded as the outcome of two main factors: first, "the structure of opportunities to which the individuals are exposed"; secondly, " their capacity to take advantage of these opportunities "; to these I would add a third, the family background. " Capacity ", as they understand it, " consists of motivation or drive as much as it consists of intelligence. But even in the U.S.A., it appears, educational achievement is to some extent dependent on the financial resources of the family; and a good deal of the drive is imparted by the family. Much is effected by " directly urging the children to achieve ". Moreover, among the working classes, " the higher the education of the parents, the more they tend to instil a desire for higher education in the children "; and in America, it must be remembered, a number of fathers in the working classes went on from high school to college; often the mothers themselves have come from a non-manual class. Above all, we are told, members of small families are more likely to rise than members of large families. For this there are several reasons. To begin with, as the authors point out, the parents who deliberately restrict the size of their families are likely to be more intelligent, better educated, more self-controlled and more ambitious to do their best for their children. At the same time the child in the small family finds it easier to pick up adult ideas, adult information, and mature ways of speech. And these differences are found not only between the manual and non-manual classes, but also between different families within the manual class.

The final conclusion that the authors draw is one which, as they put it, " has travelled some distance from the viewpoint of the nineteenth century ". " Our findings," they say, " suggest that, at the basis of our social structure, the problem is not merely one of 'natural endowment thwarted by social inequality', as was previously assumed ". In a country which is " second to none in its concern with mobility " it seems reasonable to demand that if, after all, individuals differ as widely in their intellectual endowment as many geneticists are beginning to believe, then each should have an occupation suited to his abilities. Dr. Lipset and Dr. Bendix, however, believe that their review reveals that the apparent misfits and the dissatisfactions that result are due not so much to traditional prejudice or to the natural jealousy which in a democratic country the less fortunately endowed may feel towards those whose talents secure a quick advancement; they are due rather to inadequate motivation, particularly to " a lack of interest in educational and occupational achievement ". And here the authors believe lies one of the most urgent problems for future research.

M. Howard

Readings in Social Psychology. Edited by E. E. Maccoby, T. M. Newcomb and E. L. Hartley. London: Methuen. New York: Henry Holt, 1959. Pp. xii + 674. 55*s*.

This is the third edition of a volume which first appeared just after the second world war. Since McDougall published his *Introduction* half a century ago, the interest in social psychology has developed at an ever-increasing pace. Quite half the material that appeared in the second edition of these 'readings' had not appeared when the first edition was published; and nearly a third of the present edition has been selected from the voluminous mass of literature that has been issued during the last five years.

The book makes no claim to cover the whole field; it is intended, we are told, merely as " an illustrative selection of empirical studies and approaches which may supplement more systematic presentations ". The extracts are almost entirely confined to American

[1] See C. Burt, 'The Gifted Child', *Yearbook of Education*, 1961.

writers. Readers in this country will miss references to problems which have formed major subjects for research among British psychologists—delinquency and crime, the effects of poverty and slum conditions, social mobility, and above all the influence of genetic factors. " Personality characteristics ", says one of the contributors, " develop as a function of the environment "; and that is the standpoint adopted by most of the contributors. The idea that part at least of the manifold differences in intelligence or temperament exhibited by different individuals and by different social groups might conceivably be the result of differences in innate make-up is tacitly dismissed as if it were an out-of-date superstition. On the other hand, the British reader will find that the book provides a most valuable survey of the main fields of recent American work.

Of the newer topics which found no representation in the earlier edition the most important is that of ' perception '. Two of the thirteen main classifications into which the whole book is subdivided are now concerned with this topic; and Jerome S. Bruner contributes a highly instructive article on ' social psychology and perception ', specially prepared for the present volume. Those who are not already acquainted with this new line of interest will be surprised to find that so ancient a topic bulks so large. The explanation is, not merely that older psychologists were more interested in the physical determinants of perception than in its social determinants, but that the word is here used in a very broad sense: it includes, not only what old-fashioned writers called ' apperception ', but almost every type of ' interpretation '—including, for instance, the use of such formulae as $s = gt^2/2$ to describe the course of falling bodies. The emphasis on a term like ' perception ', which seems to savour more of the earlier introspective psychology of this country than of the rigorous behaviourism of American writers, is perhaps an indication that the social psychologist has at length discovered that for his purposes a rigorous behaviourism is not enough.

Other new topics to which increased attention is devoted are those of public opinion and group dynamics. One far-reaching innovation, however, is not represented here by any addition to the content: during recent years researches in social psychology have tended more and more to employ rigorous experimental design and advanced statistical techniques. The editors comment on the change; but they feel that the students for whom they are writing will not have the technical background needed for understanding these more scientific procedures and the arguments and conclusions based on their use. They have, they say, " attempted to eliminate high-level statistics, such as analysis of variance and factor analysis "—techniques which originated in Britain and which have therefore formed favourite tools with British investigators. At the same time it should perhaps be acknowledged that this strong preference for quantitative techniques has sometimes closed our own eyes to many wider problems and many alternative modes of approach. Hence for many British readers such a book as this may perhaps come as a welcome eye-opener.

J. M. Davis

The Future of Man. By P. B. Medawar. London: Methuen, 1960. Pp. 128. 10*s.* 6*d.*

This book contains the Reith lectures broadcast during the winter of 1959. An appendix of notes has been added with detailed evidence and up-to-date references for the views advanced in the text; and this in many ways forms the most valuable part of the volume. The somewhat ambitious title was apparently chosen not by the lecturer but by the B.B.C. Professor Medawar begins with an introductory talk stressing the fallibility of all predictions about ' the future of man ', and goes on to discuss the limits that are set to future improvement by biological conditions. This is followed by a lucid and authoritative account of the ' genetic system of man '—a section which every student of psychology, whether or not he is familiar with the biological approach, should certainly ponder.

To readers of this journal the lectures of greatest interest will be those concerned with mental inheritance and the genetics of intelligence. In this field, as Professor Medawar reminds us, confusion has reigned until quite recently, because most psychological writers have clung to the theory of blending; and the few who have rejected it have assumed that the simple form of Mendelian inheritance—so-called ' segregative ' or ' unifactorial inheritance '—is the sole or at least by far the commonest type. In point of fact variations in quantitative characteristics such as intelligence and stature depend in the main on multifactorial, or, as Professor Medawar prefers to call it, on ' metrical ' or ' polygenic ' inheritance. In spite of the far-reaching influence of environment, he is fully agreed that " differences in intelligence are strongly inherited ".

In the appendix Professor Medawar refers to the ' naïvetés ' of certain psychologists, and criticizes what he calls " Burt's belief that the aptitudes revealed by intelligence tests are inborn and virtually uninfluenced by upbringing and environment ". I find it difficult to discover how any words of mine could have suggested this interpretation. Nevertheless two or three of my recent critics have quoted this passage as showing (in the words of one of them) how " a leading biologist has rejected the conclusions drawn by Howard, Burt, and others in their attempts to apply a multifactorial hypothesis to intelligence ". As a matter of fact in the notes to his lecture on ' Intelligence and Fertility ' Professor Medawar cites a pamphlet of my own with the same title; and, so far as I can see, the views which I there expressed are very similar to those that he himself reaches in his admirable discussion of the problem. I myself referred to the criticisms commonly passed on " psychologists who naïvely assume that the tests measure pure native ability "; and I stated that, to the best of my knowledge, no competent psychologist had ever entertained such a notion. All admit " the I.Q.'s furnished by intelligence tests are affected in *some* degree by schooling and other environmental influences ". The crucial question therefore is—in what degree ? The answer, I maintained, must usually depend on the nature and number of the tests employed and the type of population tested: with group tests, mainly verbal and applied once only, the influence will be rather large; if the crude results are checked and corrected by individual tests and repeated examination it will be much smaller. In general, I suggested, we might assume that " one half of the variance at the very least is attributable to ' nature ' as distinct from ' nurture '." My main evidence was based, not only on the correlations obtained between ordinary children and their parents, but also on intensive studies of " children brought up in residential institutions " and of " identical twins, particularly those reared separately from early years ".

Each type of evidence has been severely criticized both by behaviourists and by environmentalists. Professor Medawar, however, accepts all three lines of argument as valid. " The study of correlations in the population generally ", he concludes, " and particularly of their values in foster-children and identical twins reared apart, suggest that not less than half the observed variation is an inborn variation ". On the final issue therefore biology and psychology seem in complete accord.

He then goes on to discuss the possibility of a gradual decline in national intelligence. " The argument for a decline ", he says, " is based on the belief that differences of intelligence are under the control of a multitude of genes, and it is assumed that their contributions to intelligence are additive." There is, he continues, no reason to doubt that, over the normal range, inborn differences of intelligence result from polygenic transmission. This provides an emphatic reply to those who have condemned the multifactorial theory. The additive nature of the various contributions may, he thinks, be rather more open to question; but, after reviewing the evidence, he accepts this assumption also. And his final conclusion is that, " if innately unintelligent people continue to have larger families, then, with some qualifications, we can infer that the average level of intelligence will decline ", though, he contends, " the decline will be a slow one ".

His last lecture returns once again to his main theme—" the future of man ". Here he describes " the origin of a new, non-genetical system of evolution based on properties

of the brain "—namely, its ability to learn and hand on what it has learned. In a sense this new mode of evolution might be termed Lamarckian: socially, though not genetically, we do inherit acquired characters. Much of our future evolution, so far from being fatally fixed, is in our own hands; it is, or can be, " the subject of human decisions and of acts of mind ". Nevertheless, this does not mean that all inequality between individuals and groups can be abolished by a series of social and political reforms. " Mendelian heredity ", which applies, so far as we know, to the whole of genetic inheritance, itself involves a " conservation of inborn diversity ". " It works in such a way that the proportion of individuals having one genetic make-up or another can be assumed to remain pretty constant from generation to generation, unless something special happens to prevent it." Thus " a large part of human fitness is vested in a mechanism which provides for a high degree of genetic inequality and inborn diversity, which makes sure that there are plenty of different kinds of human beings, and sets a limit to any theoretical fancies we may care to indulge about the perfectibility of man ". We can thus improve upon nature; but the very possibility of doing so " depends on our continuing to explore nature ". CYRIL BURT

Principles of Comparative Psychology. By R. H. WATERS, D. A. RETHLINGSHAFER and W. E. CALDWELL. New York: McGraw-Hill Book Company, 1960. Pp. 453. 62*s*.

This volume is the joint work of seventeen specialists, each of whom deals with his own particular branch. As the preface emphasizes, the subject, unlike that of many other works with a similar title, is not animal psychology but comparative psychology in the literal sense, that is, " a comparative study of the psychological similarities and differences between species". After a brief introduction defining in greater detail the problems to be examined, the book opens with a discussion of classification. At present, as the authors rightly point out, there is nothing like an acceptable taxonomy of behavioural characteristics. However, although they themselves use terms familiar to the statistical psychologist—'correlation', ' levels ', ' dimensions showing quantitative variation ', and the like—they seem to use them merely in a descriptive or metaphorical sense. They mention one tentative scheme—very similar to the ' hierarchical ' classification of human characteristics favoured by British factorists—the scheme that begins with a twofold vertical classification into cognitive and motivational characteristics, and then introduces horizontal subdivisions for the sensory, perceptual, associative and other levels. But the suggestion appears to rest merely on an unconfirmed impressionistic basis. In this country, however, a more exact approach has been proposed by one or two psychologists, and welcomed by biologists like Julian Huxley, namely, a systematic application of the formal methods of factor analysis.

The book includes what the editors call " a new look at the old problem of innate behaviour ". Dr. King and Dr. Nichols, who contribute an admirable chapter on this subject, begin by quoting Asch's account of the unfortunate consequences that followed the type of physiological approach adopted by the behaviouristic school in the earlier decades of the present century. " The aim ", he says, was to build up a psychology " based predominantly on the study of lower organisms and to generalize the resulting principles to the human level "; and the outcome was, " a radical omission or devaluation of specifically human characteristics". Behaviour was reduced to two types—reflex activities on the one hand and activities that were supposed to be wholly learned on the other. The concepts of instinct and of innate tendencies or abilities were rejected or denied; and as a consequence, we are told, even in comparative psychology " the major research emphasis in this country " (the United States) " has been for the past twenty-five years on learning and motivation ". This they deplore; and they support their arguments for the new look by an excellent summary of recent work on innate behaviour carried out by Lashley, Lorenz, Tinbergen, and many other experimentalists.

Dr. Fuller deals with recent work on genetics and individual differences. He too insists on the value of a more intensive study of the innate components of animal behaviour.

He explains and illustrates the techniques available, and summarizes the conclusions already reached. " The inference from this array of experiments ", he declares, " is that positive results are obtained rather easily when experiments are properly designed; and the conclusion that hereditary differences have widely ramifying effects on many aspects of behaviour is strongly supported."

Here and elsewhere stress is laid on the growing recognition of the value of statistical and quantitative procedures, and special attention is paid to the ingenious statistical theories of perception and learning recently put forward. Nearly all the chapters are illustrated by diagrams and photographs; and to each is appended an up-to-date list of references.

G. R. ROBINSON

Physics and Philosophy: The Revolution in Modern Science. By W. HEISENBERG. London: Allen & Unwin, 1959. Pp. 176. 15*s*.

A glance through the earlier volumes of this journal is sufficient to show how surprisingly indifferent most British psychologists seem to be towards the remarkable revolutions in scientific thinking that have been introduced by modern physicists during the past twenty-five years. And yet, as one or two of the papers contributed to the general discussion amply indicate, the discoveries and conclusions of relativity and of quantum theory in its second phase have a close bearing, not only on such concrete problems as the functioning of the brain and the mutation of genes, but also on the more abstract issues regarding the nature and function of consciousness in both its cognitive and its conative aspects. The study of quantum phenomena, we have been told, " has brought to light a number of non-classical properties and principles which characterize ultra-microscopic phenomena—indeterminacy, complementarity, the Pauli principle, the need for partial second quantization, and for abstract representation in higher complex spaces with a high degree of invariance, and finally the amazing pluralistic world of nucleons, high energies, transient particles, and ' strong ' and ' weak ' interaction—all flying in the face of the basic assumptions of all nineteenth century science ".[1] Added to this there is the curious fact that the mathematical and statistical methods now used in quantum theory bear a singular resemblance to those employed in statistical psychology.

And, as if to clinch all this, we now have an illuminating survey of the whole field from the pen of the most eminent of living authorities on quantum physics—one who has himself been responsible for some of its most revolutionary principles and techniques. In an earlier volume, *The Physical Principles of Quantum Theory*, Dr. Heisenberg gave us a systematic survey of the chief experimental results and the main theoretical inferences; and here in his latest work his purpose is to consider " how these new conclusions permit or require man to think of himself in relation to his universe ? " The substance of the book originally formed the Gifford Lectures delivered in 1955-6 at the University of St. Andrews. The whole is prefaced by a long but admirable introduction by Dr. F. S. C. Northrop, Professor of Philosophy at Yale.

Dr. Heisenberg begins with a brief history of his subject; and for this he goes back to the beginnings of the atomic theory, as propounded by the early Greek materialists in the 5th century B.C. He reviews the general development of physical science in successive stages —up to and during the Renaissance and then from Descartes and Newton down to Einstein. At each stage he brings out in clear relief the influence of current physical theories on the philosophy of the day and the bearing of current philosophy on the physical theories. Two chapters are devoted to stating and defending the Copenhagen interpretation of quantum phenomena—an interpretation which gave an entirely new turn to quantum theory.

Dr. Heisenberg himself now tells us in his own vivid way how one day in 1926 discussions with Bohr at Copenhagen went on until late at night: " at the end I went alone for a

[1] C. Burt, ' Quantum Theory and the Principle of Indeterminacy ', this *Journal*, XI, 1958, pp. 75–93; cf. *ibid.*, 31f., XII, pp. 153f., and XIII, pp. 185f.

walk in the park and asked myself again and again: ' Can nature be so absurd as it seems in these atomic experiments ? ' ". The solution that he reached is well known: " If ", he says, " one takes account of the limitations expressed by the ' uncertainty relation ', the contradictions disappear ". And he now goes on to argue that the ' uncertainty principle '—coupled with the surprising discovery that matter, so far from being conserved, can be either ' created out of energy ' or ' annihilated into energy '—really leads us back to the old Aristotelian doctrine; " the matter of Aristotle, which is mere ' potentiality ', can be compared to our concept of energy, which gets into ' actuality ' by means of the ' form ', when the elementary particle is created ".

To the psychologist the most interesting chapter is that on the " Relation of Quantum Theory to Natural Science ". I would humbly suggest that those who have followed the long controversies in this journal with the more thoroughgoing representatives of mechanism and behaviourism, like Dr. Taylor and Messrs. Clarke, Hammond, and Hansel, might fruitfully study Dr. Heisenberg's discussion of the problem. Dr. Heisenberg himself ruthlessly rejects the nineteenth century doctrine that science must postulate universal causation. However, Professor Northrop proposes a convenient compromise based on a verbal distinction: *all* causal systems, he suggests, need not necessarily be deterministic systems; but every deterministic system is necessarily causal. Thus he would maintain that, although in the physical sciences ' probable predictions ' must now take the place of ' exact and certain predictions,' we can still call the process ' causal '.

When we turn from the physical to the biological sciences, we find, says Heisenberg, that " something has to be added to the laws of physics and chemistry before biological phenomena can be completely understood ". For example, living organisms " display a degree of stability which a complicated structure of many different types of molecules could not have on the basis of physical and chemical laws alone ".

The problems of psychology he illuminates by invoking Bohr's principle of complementarity. The consequence is, as Northrop points out, that the " Cartesian theory of a mental substance " and the dualism which this theory implies " come off very much better in this book than in any other contemporary work of the philosophy of physics ". Yet here too Heisenberg is inclined to revert to the older Aristotelian conception of the soul. " This ", he says, " as found for instance in the philosophy of Thomas Aquinas was more natural and less forced that the Cartesian concept of the ' res cogitans ' ". Those psychologists who maintain that it is " the duty of a scientific psychology to explain psychological phenomena in the terms of the physical behaviour of physical entities " should note that the physicists whose lives have been spent in studying " the physical behaviour of physical entities" are the first to reject all such maxims. For the physicist, so Heisenberg tells us, mind is a kind of mirror in which he sees the material world; and yet the modern science of psychology " seems to pay more attention to the neuron's mechanical properties than to its optical properties ". After all the mind is not merely a machine: it is an organism which cognizes.

The book concludes with a couple of chapters on ' language and reality ' and ' the role of modern physics in human thinking '. All our experiments, we are reminded, must begin and end in observable ' facts ' such as form part of the phenomena in our everyday macroscopic world. It is characteristic of such facts that they can be described in terms of common concepts and ordinary language. But our common concepts and our ordinary language " cannot be applied to the structure of the atom "; that can only be discussed by means of mathematical symbols and mathematical arguments. Thus " in our experiments about atomic events we have to do with phenomena quite as real as any in daily life; but the hypothetical atoms and elementary particles are not so real: they form a world of potentialities or possibilities rather than part of the world of facts ".

There are two further differences between classical and contemporary physics which deserve the attention of the psychologist. Nineteenth century science assumed that the physical universe could be studied as an isolated system quite independent of the observing

mind which studies it. Quantum theory and relativity have demonstrated that, since the observer himself forms part of the same system, his standpoint and his observations on the system must—at least in principle—always form parts of the total scientific description. There is even in the purest of physical investigations a psychological element which defies elimination. Secondly, it used to be maintained that science could and should be studied in complete independence of philosophy; but Heisenberg has little difficulty in demonstrating that physics is bound to introduce more physical and philosophical assumptions than the facts alone can warrant or supply. And the most fundamental of these are philosophical assumptions. They are, as the foregoing summary indicates, partly ontological—referring to the subject matter of physics in itself—and partly epistemological—arising out of the cognitive relation between the scientist as observer and the physical system that he is endeavouring to observe. Thus contemporary physics have brought about " a drastic revision of man's conception of the universe and of his own relation to it "; and the change that has thus taken place " is not simply a continuation or development of the previous picture; it involves a real break in the structure of modern science ".

The whole book is highly readable. It requires little or no knowledge of the technicalities of mathematics or physics; and it certainly deserves to be studied by every psychologist as part of the background of his scientific training.

W. L. Gregory

Abilities of Twins. By T. Huésn. *Scand. J. Psychol.*, I, 1960, pp. 125–35.

This article, which appears in a new journal, forms a valuable supplement to the monograph reviewed above. From the results of a " nation-wide programme of achievement tests " Dr. Husén has selected data relating to 634 pairs of twins. With every test their average was 1/4 to 1/3 S.D. below that of the single borns. For identical twins the intra-class correlation was 0·85 to 0·90; for non-identical twins of like sex 0·74, and for those of unlike sex 0·54. As Dr. Husén points out, the higher figures might be due to similarity in the environment. It is to be hoped that he may later be able to supply us with figures for twins reared apart.

The graphs and detailed figures for tests will be of special interest to workers in this field. In the main the results thus obtained with school children confirm the conclusions drawn in Husén's researches on adults. The distribution-curves, however, demonstrate that " the previous assumption that the inferiority was accounted for by a higher frequency of defectives proved invalid ".

Several correspondents have asked for a proof of the formula (p. 81) used for distinguishing identical twins. Professor Burt explains that the problem is a case of the ' combination of testimony '. " In a bag containing balls the proportion of white to black is p_0 to $1-p_0$. Let the relative frequency with which the testimonies of the first and second witnesses are correct be p_1 and p_2. A ball is drawn: if both say it is white, what is the probability that this is true?

" Of the 8 *a priori* hypotheses only two are relevant: (A) the ball is white and both witnesses are correct; (B) the ball is black and both are incorrect. The probability of A is $p_0p_1p_2$, of B $(1-p_0)\ (1-p_1)\ (1-p_2)$. Hence the *a posteriori* probability that the ball is white when both say it is will be

$$p=\frac{1}{1+\frac{(1-p_0)}{p_0}\cdot\frac{(1-p_1)}{p_1}\cdot\frac{(1-p_2)}{p_2}}.$$

Putting $(1-p_0)/p_0=\text{P}$, this gives the equation used by Husén in his various inquiries."

J. Conway